ENDEAVOUR

FEB 18 '58

Volume XVII Number 65

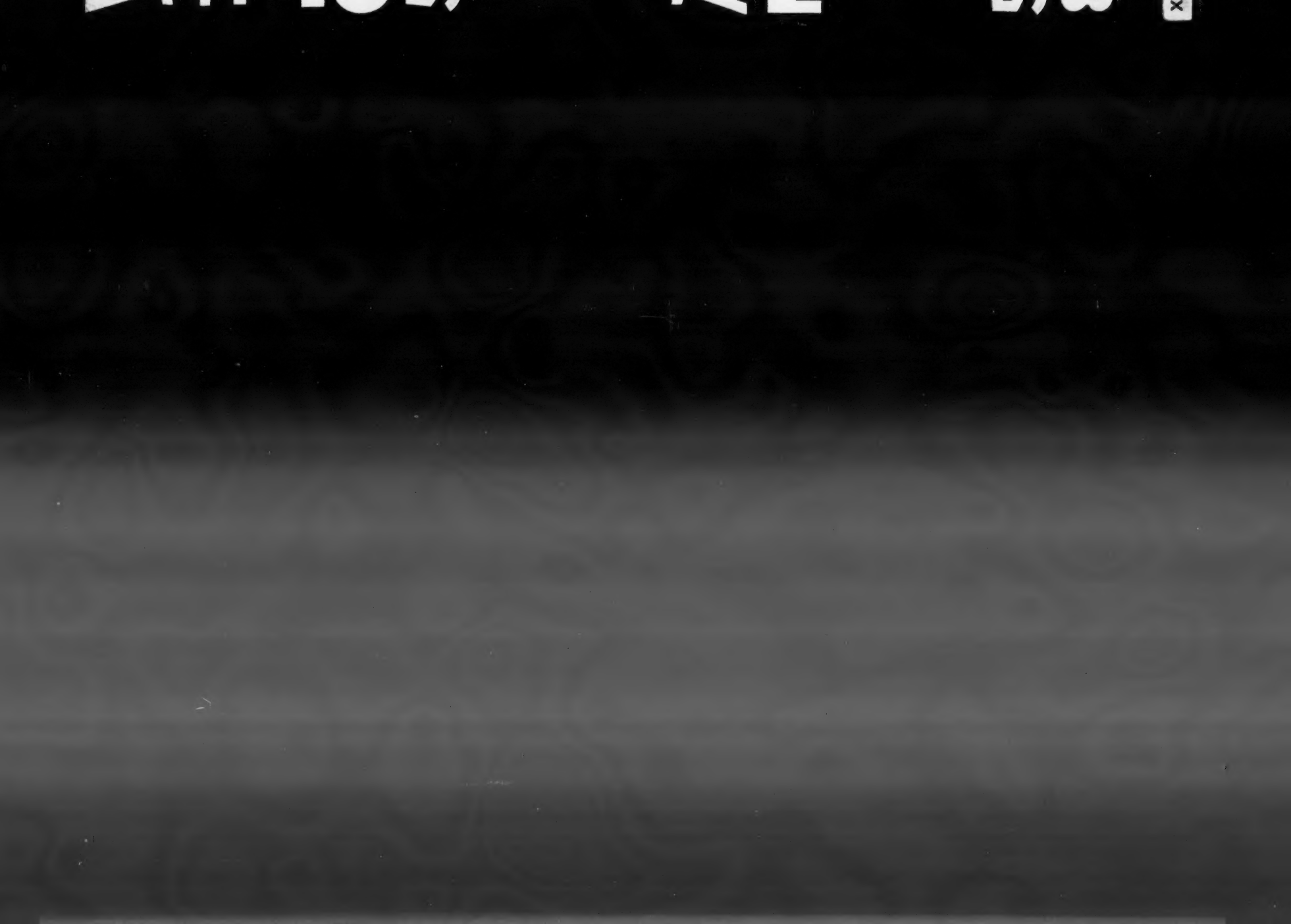
JANUARY 1958

ENDEAVOUR

The British quarterly scientific journal ENDEAVOUR was first published, by Imperial Chemical Industries Limited, in January 1942. Its purpose is to provide scientists, especially those overseas, with news of the progress of the sciences. While emphasis is laid upon British work, occasional articles from overseas contributors are included and impartial reference is made to the world's scientific literature. To make the journal truly international in character it is published in five separate editions—English, French, German, Italian, and Spanish.

No charge is made for ENDEAVOUR. It is distributed to senior scientists, scientific institutions, and libraries throughout the world, the guiding principle being that of helping scientists overseas to maintain those contacts which their British colleagues have always so much valued. Within these limits the Editors are at all times glad to consider the addition of new names to the mailing list.

The drawing on the cover is of the bark Endeavour, *which, commanded by Captain James Cook and carrying a number of scientific workers, was sent out by the British Admiralty in* 1768 *to chart the South Pacific Ocean and observe the transit of Venus*



ENDEAVOUR

A quarterly review designed to record the progress of the sciences in the service of mankind

VOLUME XVII January 1958 NUMBER 65

CONTENTS

070.432:51/54:92(415)
Editorial: Some Irish Scientists 3

523.11
Cosmology — W. H. McCREA, M.A., Ph.D., Hon. D.Sc., F.R.S. — 5

576.858.77:595.7
Insect Viruses and their Structure — KENNETH M. SMITH, C.B.E., D.Sc., Ph.D., F.R.S., and ROBLEY C. WILLIAMS, A.B., Ph.D. — 12

621.318.2:538.221(091)
The Early History of the Permanent Magnet — E. N. DA C. ANDRADE, D.Sc., Ph.D., LL.D., F.R.S. — 22

541.132
The Hydration of Ions in Solution — R. P. BELL, M.A., B.Sc., F.R.S. — 31

069.013:5/6(443)
Le Palais de la Découverte 36

532.696
Water Repellency — N. K. ADAM, M.A., Sc.D., F.R.S. — 37

591.313:541.28
Radioactive Tracers and Fertilization in Mammals — R. G. EDWARDS, B.Sc., Ph.D., and J. L. SIRLIN, Dr. Nat. Sci. — 42

049.3
Book Reviews 47

Short Notices of Books 55

Notes on Contributors 56

Some Books Received .. .. Cover iii

Editor: TREVOR I. WILLIAMS, M.A., B.Sc., D.Phil., F.R.I.C.
Editorial Assistant (Foreign Editions): G. N. J. BECK, B.Sc., A.M.I.E.E.

Imperial Chemical Industries Limited, Millbank, London, S.W.1
Communications to the Editor should be addressed to North Block, Thames House, Millbank, London, S.W.1

Some Irish scientists

The recent meeting in Dublin of the British Association for the Advancement of Science gave many reminders of important contributions by Irish scientists. The quality of their work is noteworthy for so small a country and deserves wider appreciation: this is an appropriate occasion on which to recall some of them and their achievements.

Although the family was originally a Herefordshire one, such an account may well begin with mention of Robert Boyle (1626–1691), seventh son of the first Earl of Cork and Orrery, born at Lismore Castle, Munster. His work was largely done in England, partly because he found the Ireland of his day too troubled 'to have any Hermetic thoughts in it', but he greatly influenced the course of development of Irish science. His work inspired the formation in 1683 of the Dublin Philosophical Society, which, although itself short-lived, was the forerunner of the Royal Dublin Society (1731) and the Royal Irish Academy (1785). Boyle has been fairly described as 'the father of modern chemistry': it is worth noting that the Irish chemist William MacNeven (1763–1841), professor of both chemistry and materia medica in the College of Physicians and Surgeons in New York, has been described as 'the father of American chemistry'.

Boyle's genius is hard to match in any age, but nevertheless Ireland provided a number of able successors. Notable among them was the eccentric Richard Kirwan (1733–1812), 'the Philosopher of Dublin'. Kirwan spent the years 1777–1787 in London and was there in touch with many of the most eminent chemists of the day—men such as Joseph Black (himself Belfast-born), Henry Cavendish, and Joseph Priestley—at a time when chemical thought was in a ferment. Returning to Dublin fresh from these stimulating contacts, he did much to arouse chemical interest there, especially through the Royal Irish Academy, of which he was president for the last thirteen years of his life. In London, Kirwan was a convinced believer in the phlogiston theory—which though erroneous had much to commend it in its day—and his 'Essay on Phlogiston', published in 1787, was very widely acclaimed. Lavoisier and his associates, however, had the book translated into French and then had it published with a chapter-by-chapter refutation. Kirwan accepted the refutation 'with the candour which distinguishes superior minds'—unlike Priestley, who continued to advocate the phlogiston theory until the day of his death. Kirwan's other chemical work, of a calibre to earn him the Copley Medal of the Royal Society, was concerned with such diverse subjects as chemical affinity, mineralogy, dyeing and bleaching, and agricultural chemistry.

William Higgins (1763–1825) is a controversial figure, but nevertheless a great chemist. For the last thirty years of his life he was chemist to the Irish Linen Board and, through the good offices of Kirwan, professor of chemistry and mineralogy in the Dublin Society: the latter association was a stormy one. During the past 150 years many claims have been made on Higgins's behalf that in his 'Comparative View' of 1789—primarily another refutation of Kirwan's 'Essay on Phlogiston'—he anticipated Dalton as originator of the atomic theory. The validity of these claims is hard to assess, but they are certainly not without substance: one notable authority has summarized the argument by saying that it would be fair, if not now practicable, to call the theory the Higgins-Dalton theory. It may be remarked that Higgins's ideas on the nature of the atom were undoubtedly inspired in some measure by his uncle Bryan Higgins (1741–1818), who had a chemical laboratory in Soho, London, in which William worked before going to Oxford.

Another able Irish chemist, and a notable eccentric, was Peter Woulfe (1727–1803). He too was a Copley Medallist of the Royal Society, but is best remembered by the Woulfe bottle, known to every student of chemistry, even though this was invented by Glauber. He was most probably one of the chemists before whom Dr Price of Guildford, a Fellow of the Royal Society, was in 1793 required to demonstrate his claim to be able to transmute base metal into gold. Price's nerve failed him, and he committed suicide before the demonstration.

None but the briefest list of distinguished chemists could omit Sir Robert Kane (1809–1890). By the age of thirty-five he had achieved an international reputation as a chemist—he was a Royal Medallist of the Royal Society—and then achieved equal distinction as an Irish educationist and administrator. His book on 'The Industrial Resources of Ireland' was acknowledged as masterly. Nor should James Muspratt (1793–1886) be omitted. Born in Dublin, he founded the alkali industry in England. His sons Edmund Knowles and James Sheridan were also notable chemists.

Josias Gamble (1776–1848), another great pioneer of the British chemical industry, and for a time a partner of James Muspratt, was an Enniskillen man and before settling at St Helens in Lancashire had chemical factories in Monaghan and Dublin. James Mallet (1832–1912), a graduate of Trinity College, Dublin, held a number of important chairs of chemistry in the United States. In 1882 he was elected president of the American Chemical Society, a notable tribute to a non-American who had fought on the losing side in the Civil War only seventeen years previously.

In the field of mathematics and physics, too, there have been many Irishmen of distinction. Perhaps the greatest man of science that Ireland has produced is William Rowan Hamilton (1805–1865). His early life was remarkable. At the age of thirteen he knew as many languages, including Arabic, Sanskrit, and Persian. He was elected to the chair of astronomy at Trinity College, with which went the title of Astronomer Royal of Ireland, while still an undergraduate, and while in this status had sometimes to examine graduates. His early researches led to the important discovery of conical refraction, but his great discovery was that of the calculus of quaternions in 1843. The last twenty-two years of his life he devoted mainly to elaborating this, and applying it to problems in dynamics, astronomy, and wave theory. It may be mentioned in passing that his English-born contemporary George Boole (1815–1869) founded mathematical logic while professor of mathematics at Cork: he was also the discoverer of algebraical invariants.

John Tyndall (1820–1893), born at Leighlin Bridge, was a man of wide talent, and succeeded Faraday at the Royal Institution. He is remembered most for his work on radiant heat, the absorption of heat by gases, and the scattering of light. He also made a great name for himself as a popular expositor of science. Thomas Andrews (1813–1885) of Belfast made important studies of the liquefaction of gases and was the discoverer of the phenomenon of critical temperature. Among the most brilliant of mechanical engineers was Charles Parsons (1854–1931), inventor of the steam turbine. His scientific ability was doubtless at least partly inherited, for he was the sixth son of the third Earl of Rosse, an astronomer of distinction, whose great 72-inch reflecting telescope, made in the castle workshops at Parsonstown, was for many years unrivalled. With this instrument was discovered the spiral structure of certain of the white nebulae.

The 'FitzGerald contraction' commemorates the genius of C. F. FitzGerald (1851–1901), professor of natural and experimental philosophy at Trinity College. Profoundly interested in the problem of the aether, he sought an explanation of the well-known Michelson-Morley anomaly. In 1891 FitzGerald pointed out that this anomaly disappeared if it were assumed that all bodies contracted in the direction of their motion. He developed this idea quantitatively, and calculated the theoretical degree of contraction. This conception, developed by the mathematician Joseph Larmor (1857–1942) among others, was subsequently embodied in the general theory of relativity.

Among others deserving of mention in the present context is John Joly (1857–1933), who was both physicist and geologist. He was the originator of the theory of thermal cycles, based upon the widespread distribution of radioactive elements in the earth's crust; he made a famous attempt to calculate the age of the earth from the salinity of the sea; and he was a pioneer of colour photography. G. J. Stoney (1826–1911) of Galway has a lasting memorial in the word electron, which he introduced to designate the elementary charge of electricity. R. S. Ball (1840–1913) of Dublin had a distinguished career in Ireland as an astronomer before being appointed professor of astronomy and geometry at Cambridge. He contributed notably to the theory of screws and was a brilliant expositor of science. G. G. Stokes (1819–1903) had a distinguished career in mathematics and physics and made many important contributions in hydrodynamics and in the theory of light.

This catalogue of Irish scientists includes none whose work is not generally known. To them might be added many others, familiar to the historian of science, who have made lesser but by no means unimportant contributions—T. W. Callan, R. Chevenix, T. Rutty, C. Lucas, D. Macbride, R. L. Praeger, and W. Stokes, to mention but a few names at random.

The history of Ireland has long been a troubled one and, as Boyle remarked, often not conducive to original thought. Independence, long sought, has by no means solved all problems, but the meeting of the British Association, which suggested this survey, gave a welcome opportunity of seeing how the spirit of scientific inquiry lives on despite difficulties.

Cosmology

W. H. McCREA

Astronomers are generally agreed upon some important inferences from observations of the universe. However, both optical and radio observations are still inconclusive in showing whether the universe is evolving or is in a steady state. Some observational and theoretical objections to steady-state cosmology have recently been refuted, and it appears to admit a more promising theory of galaxy formation than any proposed hitherto on the basis of evolutionary cosmology. An attempt is made here to give a brief general review of the subject.

The universe in the large is an inevitable subject for scientific curiosity, but the most commonplace physics also leads us to study it. For instance, a non-rotating frame of reference can ultimately be defined with regard to nothing less than the general background of the galaxies. This indicates at least one way in which the universe in the large influences every laboratory experiment. More generally, we conceive that the laws of physics and the actually existing physical universe must be interdependent, so that physics is bound to merge into cosmology.

We may conveniently define cosmology to be the study of the astronomical universe as the system of the galaxies and any material in intergalactic space. We are therefore not concerned with the internal affairs of any particular galaxy unless they affect the behaviour of the rest of the system or provide information about it. The present time is suitable for taking stock of the subject because there seems to be a halt in its development, although much that is happening in physics and astronomy can be seen to have a bearing upon it.

THE UNIVERSE OF GALAXIES

The lines in the optical spectra of galaxies show a shift towards the red that increases with decreasing apparent brightness. The facts are consistent, to a good first approximation, with the interpretation that the galaxies are receding with speeds proportional to their distances. This interpretation is generally accepted. It received notable support when American observers [1, 2] recently found, in the few cases so far examined, that the 21 cm line in the radio spectrum showed velocity shifts agreeing well with those in the optical spectrum. The constant of proportionality in the velocity law (Hubble's law) is written $1/\tau$; it means that if the galaxies have always had their presently observed velocities they would all have coincided in space τ years ago. The current estimate [3] of τ, based upon W. Baade's important revision of the distance-scale, is $5{\cdot}4 \times 10^9$ years. In our discussion we shall use 6×10^9 years as a round figure, since the tendency is towards larger estimates.

The fact that so general a property as the motion of the galaxies is found to be the same for all directions strongly indicates that the universe as a whole must be isotropic about our position. If the universe were not homogeneous but happened to be isotropic about ourselves, we should be in a highly favoured position. Since for philosophical reasons it is difficult to accept this, we must conclude that the universe is homogeneous, and therefore isotropic about any position. The number of galaxies accessible to present means of observation is of the order of 10^{10}. Because of practical difficulties, what is known observationally about their distribution does not yet suffice for making direct inferences. It does not, however conflict with the inference of large-scale homogeneity.

The homogeneity signifies that an observer in any other galaxy would get the same general picture of the universe as we do if the universe is in a steady state, or the same sequence of pictures if it is evolving. If valid, this is the most important thing we know about the universe. For it means that we are in a position to know all that can be known about the astronomical universe, in the sense that we could learn nothing more about its large-scale behaviour by going anywhere else within it. If the universe is in a steady state, we could moreover learn no more by living at another epoch, but this would not follow if the universe is evolving. It is in fact hard to see any justification for accepting the 'universality of physical law' unless there is also universality of physical background, i.e. homogeneity of the universe in the large. It is satisfactory to find evidence for the

latter, at any rate as regards space; whether it applies also to time is one of the main points to be discussed.

It must be remarked that the relatively small-scale distribution of galaxies is irregular. Many belong to clusters having from two or three up to about one thousand members; others cannot be assigned to particular clusters. The Milky Way Galaxy, for instance, belongs to the Local Group of some twenty members. The galaxies of a cluster are gravitationally bound to the cluster; they cannot recede from each other in consequence of the general cosmical expansion, just as the Earth cannot recede from the Sun.

THEORIES

The most obvious explanation of the apparent recession of the galaxies is that we are witnessing a one-way evolution of a universe whose contents are given once and for all. The mechanical behaviour of such a system is appropriately treated by Einstein's theory of general relativity. For simplicity, local irregularities of the contents are ignored: we consider the smoothed-out universe. The equations obtained have a range of solutions. Those that come nearest to fitting the observations all require the material to have been in a state of enormous density, infinite in the idealized model, rather less than τ years ago. The production of this singular state is given no meaning in the theory; it must be regarded as the creation of the universe, and a unique 'age of the universe' must be dated from it.

On the other hand, the essentially simplest view is that we should suppose there to be nothing to distinguish our position in time from any other, just as we have already inferred that there is nothing to distinguish our position in space. Thus, we should suppose the universe to be in a steady state. This refers to the universe in the large or to the smoothed-out universe.

The steady-state theory of the expanding universe was propounded by H. Bondi and T. Gold and by F. Hoyle in 1948. For a general account the reader is referred to Bondi's book [4]. The writer has previously reviewed the theory in this journal [5]; the arguments previously given need not be repeated, but the main conclusions will be recalled in the course of the present discussion. Numerical values in the earlier accounts have to be revised to accord with Baade's new distance-scale.

According to the steady-state theory, existing galaxies are indeed dispersing. But new matter is continually appearing throughout the universe and giving rise to new galaxies, thereby keeping the population distribution steady. Any large region contains galaxies of all ages, but with an average that works out at $\frac{1}{3}\tau$ years. The general merits of this theory are that we avoid (*a*) the dubious procedure of employing unchanging laws in a changing universe; (*b*) the paradox of finding objects such as stars that are older than the universe; (*c*) the dilemma of having everything dependent upon a creation process that nevertheless is outside the scope of the theory.

If we observe a region of the universe, say, 10^9 light-years distant, this means that we see it as it was 10^9 years ago. According to evolutionary theory we should see galaxies 10^9 years younger than our own, and so on for other distances. We ought, therefore, to discern ever earlier stages of evolution with increasing distance. Such an effect is, however, clearly impossible in a steady-state universe. The detection of any age-distance effect alone would suffice to show that the universe is not in a steady state.

Moreover, steady-state theory gives an effectively unique model of the universe. It gives only one possible relation between red-shift and apparent magnitude of the galaxies, and so on, as compared with the range of relations in evolutionary theory. Therefore, were any one of these predictions found to disagree with observation, the steady-state hypothesis would have to be abandoned. This vulnerability of the theory has been emphasized particularly by Gold.

It is consequently convenient to survey the present position mainly from the standpoint of steady-state theory, simply because it offers the most crucial predictions for discussion.

TESTS OF THE THEORIES

Suppose we had in ordinary classical space and time a uniform distribution of galaxies all of the same age and all receding from us with speeds proportional to their distances. Using classical methods, we could calculate the curves for the relations between pairs of observable quantities, e.g. Doppler shift and apparent brightness. The cosmologies under review yield curves that in general differ from those so calculated and from each other. But the corresponding curves given by the different methods all have the same tangent at the origin. Moreover, the range of present observation takes in the parts of the curves that depart from the tangent by only small amounts. Relative to available observations, therefore, all

theories make to the first order the same predictions, and discrimination between them depends upon effects of second or higher order near the limits of instrumental performance. But, because they are near these limits, the observations are subject to instrumental or selection effects that are probably of at least the same order.

Despite the superb work done by observers, it seems that, for these reasons, all efforts to discriminate between the theories by means of large-scale features must still be inconclusive. This seems to apply not only to optical observations but also to radio surveys. This is evident on comparing results by different observers [6, 7]. This limitation includes observations of properties that might reveal age-effects. Individual galaxies must evolve most rapidly in extreme youth, so that even if there is a correlation between age and distance, as required by evolutionary theory, its consequences could probably be detected only outside the range of existing instruments. The colours of certain galaxies were for a time believed to show what might be an age-effect, but the evidence has now been invalidated [8]. At present, therefore, we must consider less direct lines of evidence.

NUCLEAR SYNTHESIS

The relative abundances of the various species of atomic nuclei present in the Earth, meteorites, primary cosmic rays, solar and stellar atmospheres, and interstellar matter, show a definite correlation with nuclear properties. This indicates that the relative abundances must be the result of definite physical processes about which something ought to be discoverable. But the only known places in the universe where nuclear reactions can proceed are the stars, and until recently it was considered impossible for nuclear synthesis to proceed there beyond atomic number 8. The relative abundances were considered to be probably the same everywhere in the cosmos, apart from the effect of stellar processes upon the light nuclei. Everything, therefore, seemed to require a cosmic process of nuclear synthesis. If synthesis could not proceed in the cosmos as we know it, we had to conclude that the cosmos must have passed through a very different state in the past. The initial state of enormous density inferred from evolutionary theory seemed to provide the necessary conditions.

The theoretical possibilities of nuclear formation in the early stages of an 'exploding' universe have been thoroughly investigated, but the production of heavy nuclei could not be explained. G. Gamov, who did much of the work, now states: 'I would agree that the lion's share of the heavy elements may well have been formed later in the hot interiors of stars' [9]. This refers to a recent transformation of outlook on the astrophysical side. The brilliant investigations of E. E. Salpeter, F. Hoyle, W. A. Fowler, A. G. W. Cameron, and others have shown beyond question that nuclear synthesis can occur in stars after the exhaustion of hydrogen in the central regions [10]. There is a variety of astronomical evidence in support of the new ideas, which also tends to dispel the older view regarding 'cosmical' abundances of the elements.

The synthesis of atomic nuclei seems therefore to be a matter for individual galaxies and not to provide a criterion for cosmological theories.

FORMATION OF GALAXIES

According to evolutionary theory, galaxies must have been formed from a space-filling gas at a definite stage in the expansion of the universe. But a satisfactory process has not so far been proposed. Gamov's theory [11] demands a type of turbulence in the gas that appears to be as difficult to account for as the result it is designed to explain. D. Layzer [12] has given reasons for concluding that in an evolutionary universe the resolution of material into condensed bodies would take place in the order opposite to that usually assumed, i.e. that the gas would first produce dust, the dust would produce bodies like planets, these would produce stars, and the stars would form binaries, clusters, and finally galaxies and clusters of galaxies. While there are some broad considerations to support this idea, it has not yet shown itself susceptible of precise development.

By its nature, steady-state theory cannot pose the problem of an original formation of galaxies. D. W. Sciama has stressed that its problem is the self-perpetuation of the existing population with all its characteristics. These include the mass-function of the galaxies, the mean separation of the galaxies in space, the population-function of clusters of galaxies, the distribution of random motions of individual galaxies, and the mean density of intergalactic matter. Sciama has outlined a remarkable solution of the problem [13], which is briefly as follows.

A galaxy is, in general, in motion through intergalactic gas; this is by virtue of its random velocity and not of cosmic expansion, which applies equally to the gas. The gas that falls towards it as it passes, forms a wake behind the galaxy, as in a

well-known theory of gravitational accretion. The wake material will tend to pull itself together by its self-gravitation and so form a 'daughter' galaxy. There is a critical condition for the daughter to break away from her parent instead of remaining gravitationally bound. If they separate, the process will start again with each galaxy. If they remain together, the process will be repeated with the pair, and by the production of further offspring a cluster of galaxies of increasing population may result. But a cluster has an inherent tendency to lose members by 'evaporation'; this and other factors hinder its growing indefinitely. The galaxies that 'evaporate' off maintain the distribution of random motions of the field galaxies. Sciama finds there to be a unique and stable self-perpetuating system of this sort; it has no free parameters, and all its characteristics depend ultimately upon the properties of the atomic hydrogen which constitutes the bulk of the intergalactic material postulated in the theory. Thus we explicitly find the characteristics of the universe of galaxies to depend upon the fundamental properties of the matter from which it is constructed. The deduced characteristics are compatible with those observed. So far-ranging a theory has, of course, not yet been developed in mathematical detail, and Sciama has to employ much simplified treatments of some of the steps.

Sciama's theory cannot yet be regarded as established. But its attractiveness is obvious. It has no arbitrary feature. At the same time, every known main feature of the actual galaxies plays an essential part. By contrast, any other theory that has been proposed, even if it succeeded in explaining some features such as the masses, would apparently require a more or less independent explanation of other features, such as the linear or rotational motions. Moreover, the theory automatically disposes of one criticism of steady-state cosmology, namely that this seemed to entail the indefinite growth of any particular galaxy by accretion of the postulated intergalactic matter. On Sciama's theory a galaxy is indeed always engaged in accreting this matter, but this is the very process of forming new galaxies and not of growth of the given galaxy.

INTERGALACTIC MATTER

The steady-state hypothesis demands a modification of the ordinary form of the law of conservation of matter. But the modification is vastly less than the margin of accuracy to which this, or any other 'law', has ever been verified empirically. In fact we require the continual creation of new matter at the rate expressed by

$$\frac{\text{Matter created per year}}{\text{Existing matter}} = \frac{3}{\tau} \simeq \frac{1}{2 \times 10^9} \quad \ldots.(1)$$

This means that existing matter moves out of any region as a result of cosmical expansion, and is replaced by 'new' matter, once in about 2×10^9 years on the average.

In this and the two following sections we assume that creation does occur at the required rate, and we pursue its consequences.[1]

The new matter maintains the supply of raw material for new galaxies. It follows that

(*a*) it must provide predominantly hydrogen gas;

(*b*) its creation must not be restricted to the vicinity of existing galaxies.

Hoyle's original theoretical mean density ρ for the steady-state universe was

$$\rho = (\tfrac{8}{3}\pi\gamma\tau^2)^{-1} \ (\gamma = \text{gravitational constant}) \ldots(2)$$

and almost all other treatments reproduce this. Using

$$\tau = 6 \times 10^9 \text{ years} \simeq 2 \times 10^{17} \text{ seconds} \ldots.(3)$$

we find

$$\rho \simeq 5 \times 10^{-29} \text{ g cm}^{-3} \simeq 3 \times 10^{-5} \text{ hydrogen atom cm}^{-3} \quad \ldots.(4)$$

The amount of matter in the form of galaxies is usually estimated at an order of magnitude less than this (see [14] where (§133) a somewhat different distance-scale is used, but this will not much affect the ratio of the amounts).

These figures imply that some 90 per cent of all matter is intergalactic. The exact value of the ratio is not important for the moment; what is important is that a preponderance of intergalactic matter is admissible.

We obtain a self-consistent picture if we further infer that

(*c*) the intergalactic matter is, approximately, uniformly distributed through space;

[1] On account of the equivalence of mass and energy, continual creation may be said to involve, to the small extent stated, a modification of the first law of thermodynamics. It is this which keeps the mass or energy steady in any region. Continual creation involves a corresponding modification of the second law of thermodynamics, keeping the entropy steady in the region. But here we need not consider this further.

(*d*) this distribution is maintained by the creation of new matter, occurring approximately uniformly through space.

These inferences are consistent both with (*b*) and with Sciama's theory. Also, because of the preponderance of intergalactic matter, they imply that

(*e*) creation occurs where most of the matter is.

Thus we need not assume that the appearance of new matter is unconnected with the presence of existing matter. All that it seems necessary to exclude is such a dependence upon existing matter as would cause a continual mass-increase of each galaxy.

PHYSICAL STATE OF INTERGALACTIC MATTER

All physical processes in intergalactic matter must be extremely slow, because of the long mean free paths. But we have seen that the matter is replaced on the average in about 2×10^9 years. It follows that processes that take longer than this to produce appreciable effects are not significant. The writer is not aware of any published theory of the physical state of intergalactic matter taking account of such considerations; this and the following section are based on his preliminary results.

The creation process must be one involving fundamental particles and producing hydrogen. Consequently at some stage it must yield protons and electrons. Neutral hydrogen atoms, not being fundamental particles, can appear only through the subsequent mutual captures of protons and electrons.

It is interesting that there is no reason to suppose the universe as a whole to be electrically neutral, and the consequences of supposing otherwise would be worth examination. For the sake of simplicity, however, we shall assume equal numbers of protons and electrons. Also we will treat only the smoothed-out universe.

Let N_p, N_e, N_H = steady-state number-density of protons, electrons, hydrogen atoms. We write

$$\mathcal{N} = N_p + N_H \qquad \ldots.(5)$$

Also let $\alpha N_p N_e$ = production rate of hydrogen atoms/unit volume and n_p, n_e = creation rate of protons, electrons/unit volume.
We are assuming

$$N_p = N_e \quad \text{and} \quad n_p = n_e \qquad \ldots.(6)$$

Let O be an origin at rest in the medium and consider the material in a sphere about O of fixed radius r. Owing to cosmical expansion, the material is flowing out through the spherical boundary with radial velocity r/τ.

In the steady state, the total amount of material flowing out equals the total amount created inside, and the number of hydrogen atoms flowing out equals the number produced inside. Thus

$$4\pi r^2 \mathcal{N} r/\tau = \tfrac{4}{3}\pi r^3 n_p \qquad \ldots.(7)$$

$$4\pi r^2 N_H r/\tau = \tfrac{4}{3}\pi r^3 \alpha N_p N_e \qquad \ldots.(8)$$

From (7) we get the creation rate, in agreement with (1):

$$n_p = 3\mathcal{N}/\tau \qquad \ldots.(9)$$

From (5), (6), and (8) we get

$$N_p^2 = \frac{3}{\alpha\tau}(\mathcal{N} - N_p) \qquad \ldots.(10)$$

This is the *dissociation formula* for the material.

We assume there to be a kinetic temperature T. Also we neglect photo- and collisional ionization, as seems justifiable for intergalactic space. Then α is the usual 'recombination coefficient' for hydrogen, and we have ([14] §36)

$$\alpha \simeq 3 \times 10^{-10}\, T^{-\frac{1}{2}} \text{ cm}^3 \text{ sec}^{-1} \qquad \ldots.(11)$$

Using $\tau = 2 \times 10^{17}$ sec, $\mathcal{N} = 3 \times 10^{-5}$, as in (3) and (4) we find from (10) and (11) 90 per cent ionization at about $T = 8 \times 10^4$ degrees and 10 per cent at about $T = 12$ degrees.

Thus, over a big range of temperature, there will be significant fractions of both neutral and ionized hydrogen. Until more is known of a possible creation process, however, we cannot predict a value of T.

POSSIBILITY OF OBSERVING INTERGALACTIC MATTER

An observational determination of the contents of intergalactic space would be about the most useful possible advance in cosmology, for we have seen that these contents may form the bulk of the matter in the universe, and knowledge of their nature would help greatly in choosing between theories. Some way of making the determination will almost certainly be discovered, but unfortunately no present means is likely to succeed.

There is apparently no optical means of detecting ionized or neutral intergalactic hydrogen, for much the same reasons as those which preclude the optical determination of the general distribution of interstellar hydrogen within the Galaxy.

As regards the possibility of detection by the 21 cm radio line, owing to the cosmical red-shift, which increases with distance, intergalactic hydrogen of the estimated density has small optical thickness at any given wavelength. Consequently any absorption effect is very small. Any emission will not produce a 'line' but a band of very small, roughly uniform strength extending on the long-wave side of 21 cm. It would almost certainly be too weak to be observed.

Among possible methods to be investigated are any that might be based upon the slight electrical conductivity imparted to intergalactic space by ionized hydrogen, or upon any effects in the medium that might be produced by the outburst of a supernova in a galaxy. Any direct observational evidence for Sciama's process of galaxy production would also be evidence for the presence of intergalactic hydrogen.

It ought to be recalled that 21 cm emission reveals [15] a tenuous envelope of neutral hydrogen around the Magellanic Cloud galaxies out to diameters two or three times those of the visible parts. This is not positive evidence for a general intergalactic medium, but it would not be surprising to find such envelopes to be continuous with a general medium. A. E. Whitford [16] has called attention to the possibility of the more general detection of intergalactic neutral hydrogen within the non-expanding Local Group.

CONNECTION WITH ATOMIC PHYSICS

The writer has shown [17] that the possibility of continual creation is not incompatible with general relativity theory. F. A. E. Pirani [18] has carried the argument a stage further by showing what type of hypothetical elementary process would achieve the required result on a formally orthodox interpretation of that theory. Further, from the standpoint of nuclear physics, A. Salam [19] has remarked that, since non-conservation of 'parity' has been discovered in nuclear interactions that are 'weak' in a specific sense, the sort of non-conservation required for continual creation might apply to interactions at the next order of 'weakness'. Thus the concept of continual creation has not proved repugnant to current physical theory.

It has, indeed, provoked a further line of speculation. For 'matter', constituted of positively charged nuclei and negative electrons, and 'anti-matter', constituted of negatively charged nuclei and positive electrons, are treated in a symmetric fashion by current theory. In particular, both sorts have theoretically identical inertial and gravitational properties. On general grounds we might therefore expect any creation process to produce statistically equal amounts of matter and anti-matter and the universe in the large to be composed of equal amounts. On the other hand, any particular galaxy must be composed effectively entirely of one sort only. G. R. Burbidge and F. Hoyle [20] have shown that a galaxy might sustain at most about 1 part in 10^7 of the opposite sort. It has even been suggested that some very energetic collisions observed to be occurring in some parts of the universe are between galaxies of opposite compositions. We should thus be confronted with the formidable problem of how matter and anti-matter, intermingled in space, could be segregated to produce galaxies.

We have already noticed, however, that new matter must appear, if at all, in the presence of existing matter. The problem just stated would not arise if one sort of matter bred only its like. Then, if the universe as a whole does contain comparable amounts of both kinds, we should conclude that very large tracts contain only one sort or the other. Or the universe might 'happen' to be composed almost entirely of the sort we call matter, with anti-matter as no more than a rarity anywhere.

The questions involved here are of general physical interest, apart from particular cosmological theories, and they would arise in much the same form in any cosmology. Interest in them was reawakened by the laboratory observation of the antiproton in 1955. Another laboratory observation, that of the neutrino in 1956, also stimulated cosmological speculation [21]. For neutrinos might pervade the universe in great numbers and contribute to the general gravitational or other fields without producing any other observable effects.

REVIEW

Hubble's law of red-shifts, the homogeneity of the universe, the existence of the time-characteristic of the universe τ, the relation between the gravitational constant, the time-characteristic, and the mean density (equation (2), which follows from almost all interpretations with only minor differences in the numerical factor)—these are all discoveries of great and probably lasting importance. Beyond these, however, our survey would seem disappointingly inconclusive were it not possible to see some significance in the apparent inconclusiveness.

The steady-state theory has survived for the nine years since its announcement. Despite its peculiar vulnerability, no observation has been found to refute it that is not itself of uncertain interpretation. The observation most frequently quoted against it, the Stebbins-Whitford effect, has itself been refuted [8]. The obstacle regarding nuclear synthesis has disappeared and, with it, one of the main arguments for evolutionary theory. The solution of the problem of galaxy formation seems to be within reach of steady-state cosmology, while other theories have not shown much progress towards solving it.

If the universe has been evolving at the rate inferred from evolutionary theory, so that its age is about the same as that of the Earth, we should expect to find many results in astrophysics obviously to depend upon this fact. As it is, there is nothing in current astrophysics that can be claimed to require it. But if the universe is in a steady state, astrophysics must proceed without appeal to changing cosmical conditions: this is precisely how it is proceeding. In that case, moreover, it must be much more difficult to use astrophysics to infer anything about the cosmical conditions than it would be if those conditions were changing.

For these reasons, the present state of inconclusiveness is probably no worse than we might expect according to steady-state cosmology. It is actually evidence for the validity of the theory.

On the observational side, it has to be remembered that the study of the universe in the large is possible with only a few existing instruments, and that so far as radio-astronomy is concerned we have seen only the beginning of its possibilities. It seems as though an extension of existing work to not prohibitively greater distances would settle some vital issues. But difficult statistical problems are concerned in saying what extension is actually necessary. It is much to be hoped that some unexpected discovery will open the way to the study of whatever intergalactic material exists.

If we accept the evidence that creation has occurred at least once, it seems natural to consider the possibility of its occurring more than once. In fact, the concept of continual creation calls for investigation without, necessarily, special reference to any particular theory of cosmology. The matters briefly mentioned in the preceding section show that discoveries in laboratory physics are explicitly giving rise to cosmological problems, and especially to ones concerned with creation of matter. They also show the possible existence of factors that do not emerge from astronomical investigations alone, and that may account for some of the lack of progress in astronomical cosmology. Progress in cosmology may have to await further progress in physics.

REFERENCES

[1] LILLEY, A. E. and MCLAIN, E. F. *Astrophys. J.*, **123**, 172, 1956.
[2] HEESCHEN, D. S. *Ibid.*, **124**, 660, 1956; *Astron. J.*, **62**, 18, 1957.
[3] HUMASON, M. L., MAYALL, N. U., and SANDAGE, A. *Astron. J.*, **61**, 97, 1956.
[4] BONDI, H. 'Cosmology'. Cambridge University Press, London. 1952.
[5] MCCREA, W. H. *Endeavour*, **9**, 3, 1950.
[6] RYLE, M. and SCHEUER, P. A. G. *Proc. roy. Soc., A*, **230**, 448, 1955. (See also *Sci. Amer.*, **195**, No. 6, 10, 1956.)
[7] MILLS, B. Y. *Sci. Amer.*, **195**, No. 6, 8, 1956.
[8] WHITFORD, A. E. Reported in *Sky and Telesc.*, **16**, 222, 1957.
[9] GAMOV, G. *Sci. Amer.*, **195**, No. 3, 154, 1956.
[10] CAMERON, A. G. W. *Publ. astr. Soc. Pacif.*, **69**, 201, 1957.
[11] GAMOV, G. *Math.-fys. Medd., Copenhagen*, **27**, No. 10, 1953.
[12] LAYZER, D. *Astron. J.*, **59**, 170, 1954.
[13] SCIAMA, D. W. *Mon. Not. R. astr. Soc.*, **155**, 15, 1955.
[14] ALLEN, C. W. 'Astrophysical Quantities'. Athlone Press, London. 1955.
[15] KERR, F. J., HINDMAN, J. V., and ROBINSON, B. J. *Aust. J. Phys.*, **7**, 297, 1954.
[16] WHITFORD, A. E. *Astron. J.*, **59**, 194, 1954.
[17] MCCREA, W. H. *Proc. roy. Soc., A.*, **206**, 562, 1951.
[18] PIRANI, F. A. E. *Ibid.*, **228**, 455, 1955.
[19] SALAM, A. 'Imperial College Inaugural Lectures', London. 1957.
[20] BURBIDGE, G. R. and HOYLE, F. *Astron. J.*, **62**, 9, 1957.
[21] REINES, F. and COWAN, C. L. *Nature, Lond.*, **178**, 446, 1956.

Insect viruses and their structure

KENNETH M. SMITH and ROBLEY C. WILLIAMS

Virus research has tended to be concentrated upon forms that cause disease in man or in the crops and animals on which he depends. But such practical considerations are not necessarily important in fundamental research: for this purpose certain insect viruses, not hitherto very closely studied, seem to have important possibilities which are here discussed. A virus which infects larvae of *Tipula paludosa* has particularly interesting properties, and an ingenious shadow-casting technique has made it possible to identify its particles as icosahedra: it is the first virus in which the geometrical form of the particles has been demonstrated.

Viruses and virus diseases have been investigated by people whose aims have been fairly readily distinguishable as humanitarian, economic, or fundamental. The first aim involves consideration of the virus ailments of man and their alleviation, while the second is concerned with study of the diseases of the higher plants and animals and with reducing the losses that result from these diseases. The viruses that affect insects have not received much attention from those whose approach is either humanitarian or economic, and consequently they are not well understood from this point of view. But the third approach—the fundamental study of viruses—is concerned only with the questions of what viruses are and how they multiply within living cells. Here one kind of virus is as good as another if it promises to provide significant information. In this type of study, insect viruses may be important, and some of their known properties lead us to believe that recognition of their importance in fundamental virus research is overdue.

Although viruses attack almost every known kind of organism, there seem to be some exceptions. For example, in the plant kingdom no virus has been discovered that infects the ferns or the true fungi. In the insect world only three orders are definitely known to be susceptible to virus infection. This apparent restriction in host range may, in part, be only a reflection of our lack of knowledge, but it is so far the fact that viruses have been found only in the Lepidoptera (butterflies and moths), the Hymenoptera (ants, bees, and wasps), and the Diptera (the true flies). No virus diseases have been reported for the populous orders like the Hemiptera, which includes the aphids and plant bugs; the Orthoptera, such as grasshoppers and locusts; and the Coleoptera, containing some forty thousand species of beetles.

For practical reasons it might be distinctly important to discover virus diseases in some of the insect orders not now believed to harbour them. The deliberate inducing of virus plagues among insects has been studied for some time in the hope of exercising biological control over insect pests. Such attempts involve first collecting large quantities of some particular virus and then spraying the infectious material in trial fields where the appropriate insect pest is in the susceptible stage. Considerable success in such trials has been occasionally achieved [1], with dramatic mortality among the pests and no damage to the crops. But one of the drawbacks to the general use of this form of control is the existence of several kinds of insect pests, such as aphids and locusts, that appear to have no natural virus enemies.

KINDS OF INSECT VIRUSES

Since the symptoms displayed by insects suffering from virus diseases are rather variable and non-specific, the classification of insect viruses has been based upon their morphological and cytological characteristics. The characteristics used relate either to the appearance of the virus particles themselves, as seen under the electron microscope, or to the appearance and intracellular localization of certain inclusion bodies associated with the virus particles. On the latter basis three groups of insect viruses are now recognized, although one of the groups is commonly divided into two sub-types. The virus particles associated with any one group or sub-type seem to have a characteristic size and shape. In the few cases where the particles have been purified and analysed it has been found that they are nucleoproteins, with the nucleic acid of the deoxy type [2]. The types of disease produced by the viruses of a group seem to depend upon where the virus multiplies within the body of the host. Although there are instances of virus being recovered from

insects in the pupal, and even adult, stage [3], it is generally conceded that only the larval forms are susceptible to frank virus diseases. It is found that within the larvae many organs and types of cells are liable to infection by some virus of one of the three groups. The cells most commonly invaded are those of the skin, the tracheae, the fat-body, the mid-gut, and the circulating cells of the blood.

Polyhedroses. The largest of the three groups of insect viruses so far investigated is the one causing the polyhedral diseases, or polyhedroses. This name derives from the observation that large numbers of bodies of polyhedral shape are present within certain tissues of the infected host. In a few cases these bodies have been shown to be crystalline, as examined by X-ray analysis [4] and by electron microscopy [5]. The polyhedral crystals are not uniform in shape and size, but they are sufficiently characteristic in appearance, and large enough, to be easily recognized under the light microscope. For many years there was much controversy about the nature of the polyhedral bodies, and for some time they were generally thought to be some kind of micro-organism causing the disease. It is now known that the bodies are primarily protein crystals within which several hundred virus particles are occluded. We shall refer to true virus crystals later, and it should be noted that the polyhedral bodies, though crystalline, contain virus particles only as a minor constituent.

The polyhedral virus group is divisible into two types, in each of which the polyhedral bodies have characteristic properties and unique localization within infected cells. The nuclear polyhedral viruses usually attack the skin, tracheae, fat-body, or blood cells, and as their specific name implies, they are found to develop only in the nuclei of the cells affected. When the affected cells become grossly infected they burst and release the polyhedral bodies into the blood. These released polyhedral bodies are readily obtained in pure form by gentle sedimentation and decantation of a water suspension of the contents of the bodies of infected insects. When the polyhedra are treated with weak alkali the protein contained in them dissolves, leaving behind what appears to be an outer membrane within which are rod-shaped virus particles. They may be found either singly or in bundles (figure 1). It is possible to obtain the rod-like particles quite pure, and when this is done they are found to be highly infectious when inoculated into healthy larvae [6].

The cytoplasmic polyhedroses have been found only in the cells of the gut, usually the hind-gut, where they seem to develop solely in the cytoplasm of the affected cells. The polyhedral bodies may be extracted from the cells and purified; when this is done, the purified material proves to be infectious [7]. The morphological identification of the elementary infectious particle is not, however, as clear-cut as it is in the case of the nuclear polyhedroses. When the cytoplasmic polyhedral bodies are treated with alkali they partially dissolve, leaving behind a sort of matrix honeycombed with round holes, and a number of small, spherical particles [8]. Since these particles have not been assayed for infectivity it cannot be stated with assurance that they are the elementary infectious agent. The electron microscope reveals in thin sections of the cytoplasmic polyhedra the presence of small, approximately circular particles (figure 2) that are probably of the same nature as those extractable by weak alkali [9]. Inasmuch as sections cut through nuclear polyhedra show characteristic particles known to be viral in nature, it can be postulated, but only by analogy, that the small circular objects seen in sections of cytoplasmic polyhedra are the virus particles.

The two types of polyhedral virus disease differ in the symptoms they produce, particularly in lepidopterous larvae. The nuclear polyhedral viruses usually attack the skin, rendering it extremely thin and fragile; it ruptures at a touch and liberates the liquefied contents of the body, which consist largely of polyhedral crystals. Caterpillars that have died from a nuclear polyhedrosis are usually found in a characteristic position hanging by one or two pairs of the abdominal 'feet'. Infected caterpillars of the nun moth (*Lymantria monacha*) tend to migrate to the tops of spruce trees (their usual habitat), where they remain hanging head downwards. This rather peculiar behaviour has given rise to the German name of *Wipfelkrankheit*, or tree-top disease. In the disease produced by the cytoplasmic polyhedroses the skin is not attacked. The caterpillar dies intact and tends to dry up rather than liquefy. Towards the termination of the disease, in some cases, the cells of the gut are crammed with polyhedra and finally burst, spilling out the contents to be voided with the faeces. Frequently it is possible to see the polyhedra showing through the skin as white patches.

Granuloses. A second group of insect viruses is responsible for the diseases known as 'granuloses', a name deriving from the fact that the elementary virus particles appear to be enclosed in a

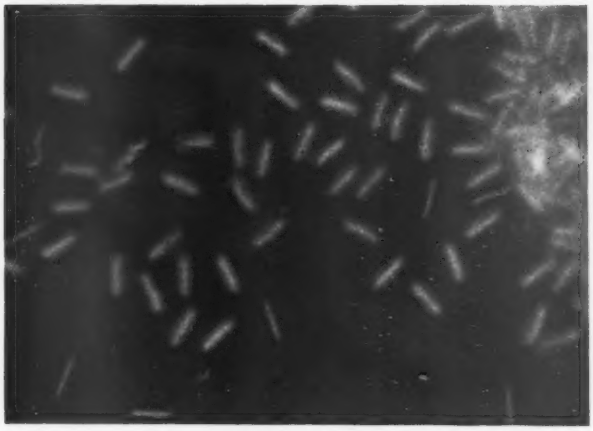


FIGURE 1 – *Nuclear polyhedral body from* Bombyx mori, *dissolved to disclose virus particles. Many are still enclosed within a membrane.* (× 20 000)

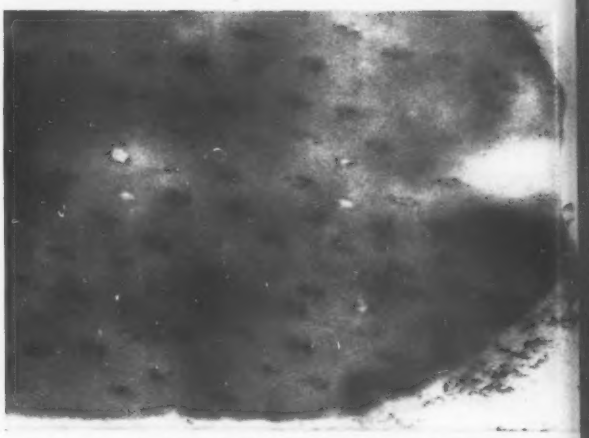


FIGURE 2 – *Section of cytoplasmic polyhedral body from* Arctia caja. *The quasi-circular bodies within are presumed to be sectioned virus particles.* (× 85 000)

FIGURE 3 – *'Granules' from a granulosis disease of* Euplexia leucipera *after treatment with alkali. Note central capsules and flattened granules. The virus particle is inside the central capsule.* (× 26 000)

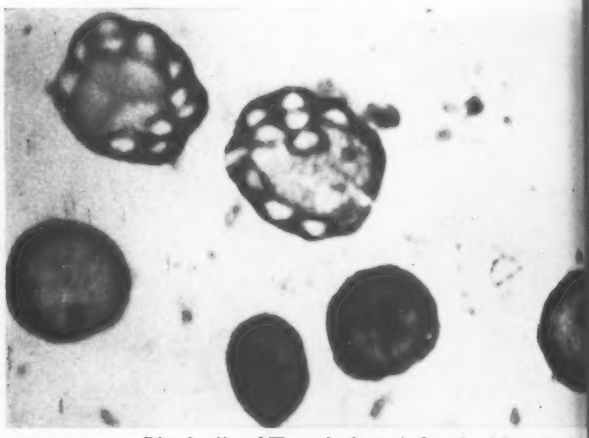


FIGURE 4 – *Blood cells of* T. paludosa *infected with a nuclear polyhedrosis. Upper two cells contain many polyhedral bodies. Light micrograph.* (× 1500)

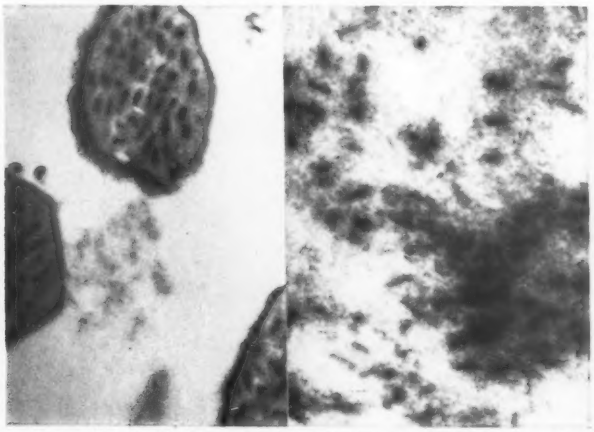


FIGURE 5 – (*left*) *Sections of polyhedral bodies from* T. paludosa, *containing rod-shaped particles* (× 24 000). (*right*) *Portion of nucleus showing similar particles, not enclosed in polyhedral body* (× 30 000).

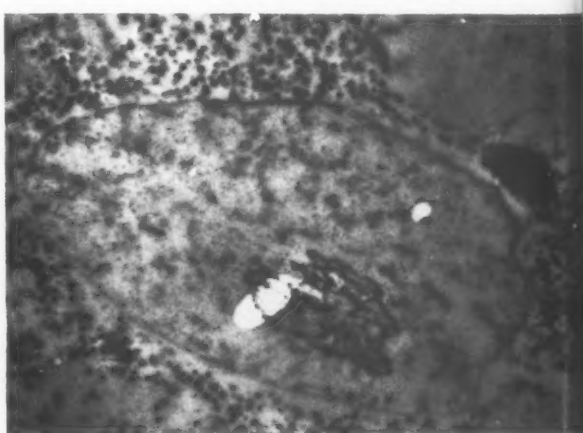


FIGURE 6 – *Portion of fat-body cell infected with* Tipula *iridescent virus (TIV). The darkly staining virus particles are solely in the cytoplasm.* (× 8500)

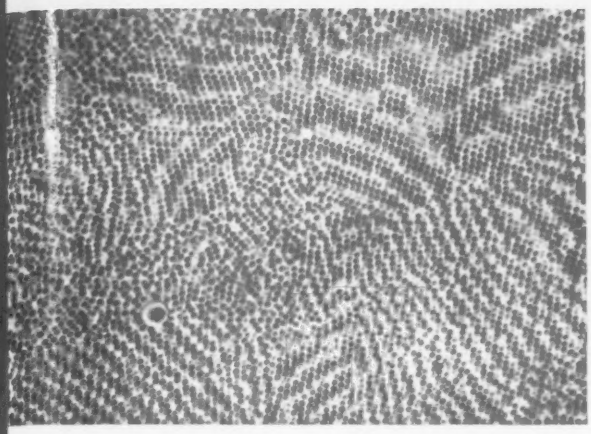


FIGURE 7 – *Pellet of TIV sectioned to show the existence of a polycrystalline array.* (× 10 000)

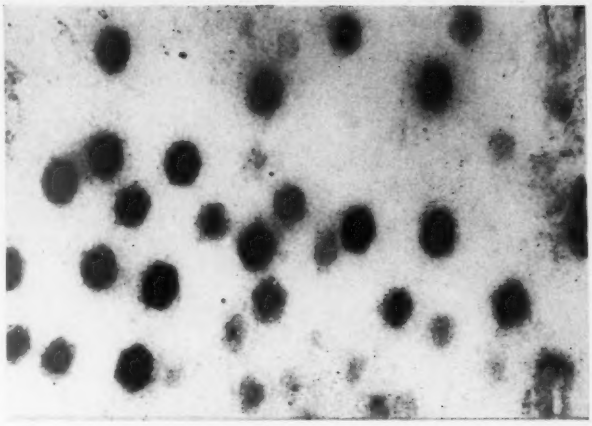


FIGURE 8 – *Sectioned particles of TIV, exhibiting some internal structure and frequent six-sided forms.* (× 55 000)

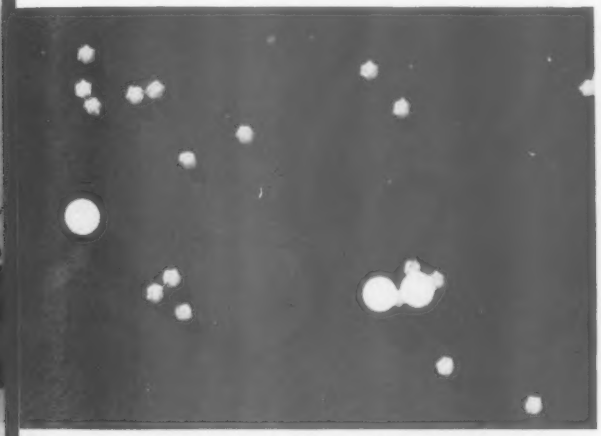


FIGURE 9 – *Air-dried shadowed particles of TIV. Note six-sided contours. Spherical objects are polystyrene latex particles for reference.* (× 23 000)

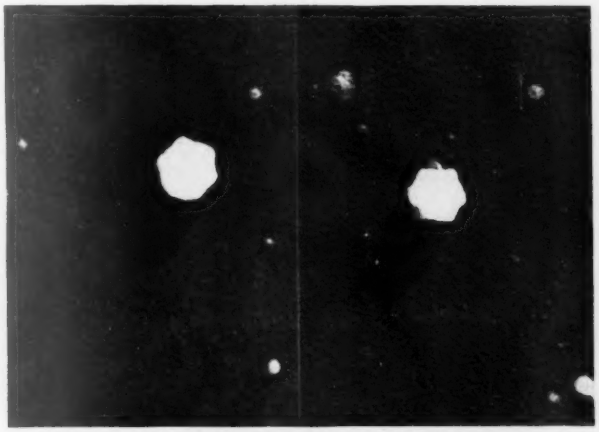


FIGURE 10 – *Two frozen-dried particles of TIV, showing that two distinct shadow forms may be cast: four-sided and pointed, five-sided and blunt.* (× 67 000)

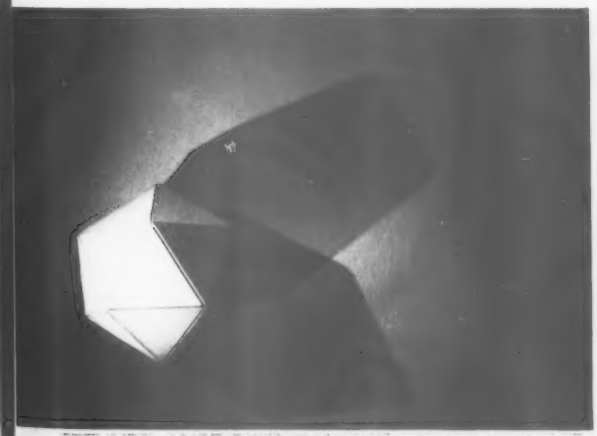


FIGURE 11 – *Model of icosahedron, doubly shadowed. Note two shadow shapes which can be cast simultaneously only by an icosahedron in this orientation.*

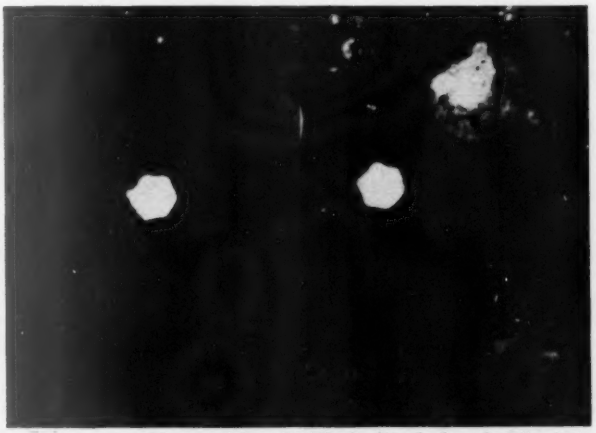


FIGURE 12 – *Two particles of TIV, doubly shadowed. The similarity between the shapes of these shadows and those of the model (figure 11) is evident.* (× 55 000)

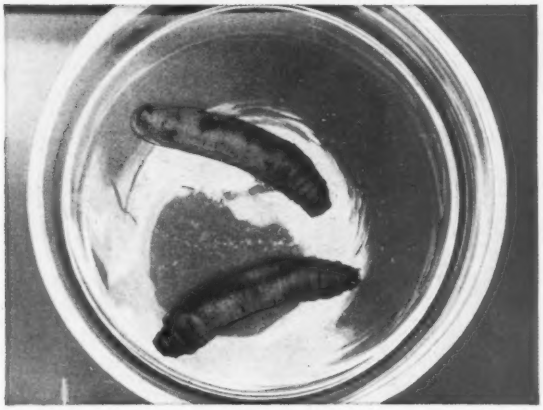


FIGURE 13 – *Two* T. paludosa *larvae severely infected with TIV. The normal tan colour has changed to a bluish grey.*

FIGURE 14 – *A* T. paludosa *larva, infected with TIV, dissected to show the blue colour of the hypertrophied fat-body.*

FIGURE 15 – *A bottle of purified TIV that has stood in the cold. Note that a layer of coloured crystallites has formed at the bottom.*

FIGURE 16 – *A centrifuged pellet of purified TIV photographed by transmitted light. The pellet appears uniformly orange.*

FIGURE 17 – *A pellet of TIV allowed to re-hydrate and photographed by reflected white light. The iridescence is evident.*

FIGURE 18 – *An array of crystals of TIV photographed by reflected white light.* (× 10)

958

TIV,
ody.

TIV
hoto-
ight.

capsule or granule. No polyhedral bodies are found in cells infected with a granulosis disease. That the encapsulated particles are the causative agents of the granuloses is established from the observation that a suspension of purified granules is highly infectious [10]. When the granules are treated with weak alkali a rather complex structure is disclosed. There is an outer capsule shaped like a prolate spheroid, and inside this there is found a similarly shaped, but separable, capsule (figure 3). Within this inner structure a rod-shaped particle is found, apparently enclosed in a limiting membrane [11]. It is not known if the innermost particle, with its intimate membrane, is infectious.

The granulosis group of viruses has so far been found to be restricted in host range to the caterpillars of Lepidoptera. Examination of thin sections of tissues of infected insects reveals bodies that are similar in shape and size to sectioned granules, and from such observations an indication can be obtained of the likely sites of multiplication of the virus [12]. The sites are chiefly the tissues of the skin and the fat-body. The progress of the disease resembles that induced by the nuclear polyhedroses, in that the body contents are liquefied and are easily liberated by rupture of the skin.

The granulosis viruses may have a practical importance because some of their hosts—like the caterpillars of the white, or cabbage, butterfly—are insects of economic importance. Since a suspension of highly infectious granulosis virus can be produced in large quantity (as much as one gallon from five infected larvae), it is likely that biological control of these lepidopterous pests can be secured through spraying infested fields.

Non-encapsulated viruses. A third group of insect virus diseases is characterized by the occurrence of infectious particles generally similar in appearance to those found in virus diseases of animals and higher plants. The virus particles are not associated with any kind of inclusion crystal or encapsulation, but rather are found free in the cells of the particular tissue attacked. Only two examples [13, 14] of this group are so far definitely known, one occurring in larvae of Diptera, the other in larvae of Lepidoptera. It is possible to extract the particles from the cells and purify them; in both cases the purified, approximately spherical, particles have been found to be infectious. One member of this group of non-encapsulated viruses, here called the *Tipula* iridescent virus (TIV) is of considerable interest, and will be described in some detail later in this article.

HOW INSECT VIRUSES ARE SPREAD

Many of the plant viruses, and some of the viruses affecting higher animals, are transferred from host to host only by means of arthropod vectors, such as aphids, mosquitos, and mites. Insect viruses are mostly, if not wholly, spread from diseased to healthy larvae by the ingestion of infected food. The efficiency of spreading is greatly increased by the way in which the virus particles are, as it were, often 'packaged for delivery', i.e. contained in granules and polyhedral bodies which may retain their infectivity for several years. On the other hand, the particles of the nuclear polyhedral viruses are quite labile after removal from their protein matrix. The manner of development of the nuclear polyhedroses is also favourable towards the spread of disease; the skin of the diseased insect ruptures on death and the liquefied contents of the body get splashed by rain upon plants used for food by healthy larvae. In addition, in some cases the healthy larvae are strongly attracted by the liquefying cadavers, on which they feed with devastating results.

The way in which the cytoplasmic polyhedroses develop, however, is not so favourable to the spread of these diseases. Caterpillars infected with this type of polyhedrosis tend to dry up rather than liquefy, and on death they retain their body structures intact. It is possibly for this reason that these diseases are found to spread more slowly than do the nuclear polyhedroses. The method of spread of the cytoplasmic polyhedra is probably by ingestion of faeces contaminated in the late stages of the disease by polyhedra liberated into the lumen of the gut.

There is very little information about how the viruses that lack inclusion bodies are spread, but presumably they are in some manner ingested with the food. It is a little difficult to visualize how the *Tipula* iridescent virus gets about, since its specific host, the larva of *Tipula paludosa* (the crane fly or daddy-long-legs) is a soil-inhabiting insect. No doubt in this case also the food becomes contaminated, but the comparative rarity of this virus disease may perhaps be accounted for by difficulty in dissemination.

There is good evidence that some of the insect viruses are passed through the egg to succeeding generations. Examples of this type of passage are found in a cytoplasmic polyhedral virus from *Arctia caja* (the garden tiger moth) [15], and in a

granulosis virus affecting the caterpillars of *Pieris brassicae* (the large white butterfly) [16]. This mode of dissemination of disease is of practical importance in some cases where biological control of insect pests is being attempted, since passage of virus through the egg allows us to infect future generations, which is not possible with insecticides. The phenomenon of the passage of a virus to the progeny of infected insects brings us to the consideration of 'latency' in insect viruses.

LATENT VIRUSES

Many examples are known of viruses that may remain dormant in their hosts for prolonged periods, even through successive generations, and that suddenly begin to cause pathological changes in the absence of any overt inoculation of the host. In their dormant stage such viruses are said to be 'latent', an indefinite term indicative of our ignorance of what is happening. Latency is found throughout the virus diseases of bacteria, plants (where it is quite common), the higher animals, and insects. It is encountered with great frequency in the virus diseases of insects, where in some populations almost every individual appears to carry a latent virus. Needless to say, the ubiquitous presence of virus latency among insects renders the precise biological assay of their viruses almost impossible.

It is sometimes possible to cause a latent virus to become virulent by artificial means, such as by rearing under unsuitable environmental conditions and by feeding with chemicals. One of the most successful methods is to introduce into the insect a virus that may in the event prove to be different from the one being carried in the latent form. It has frequently happened during cross-inoculation studies that an insect develops a virus disease different from the one with which it was inoculated. Inoculation with a nuclear polyhedral virus frequently stimulates the development of a cytoplasmic polyhedrosis; but, oddly enough, the reverse seems never to occur. Once a latent virus has been transformed into a virulent one it can be transmitted from insect to insect. This circumstance may have a practical application in some cases: an experience with the larvae of the winter moth, *Operophtera brumata*, provides an example [16]. After a search for a naturally occurring virus affecting this species had failed, some apparently healthy larvae were inoculated with a nuclear polyhedral virus from the caterpillar of a butterfly, *Vanessa cardui*. A very high mortality resulted, all the larvae dying, but they succumbed to a cytoplasmic polyhedrosis. Once this virus had been established in virulent form it was transmitted successfully to numbers of winter moth larvae, allowing a stock of virus suspension to be built up for use in experiments on biological control.

TWO INTERESTING INSECT VIRUSES

Although very few virus diseases have so far been found in the larvae of Diptera (flies), two have such interesting particles associated with them that they merit a detailed description. Both diseases affect the larvae (popularly known as leatherjackets) of *Tipula paludosa*, and both may occur simultaneously in the same insect. One virus causes a polyhedral disease of a type unusual in that the associated polyhedra are not soluble in the usual reagents [17]. Affected larvae exhibit a characteristic pallor that is apparently due to an abnormally large number of blood cells having nuclei largely filled with polyhedral bodies. The virus seems to multiply within the greatly enlarged nucleus of the blood cell; as the disease progresses, the polyhedral bodies begin to form around the periphery of the nucleus in close proximity to the nuclear membrane (figure 4).

As has been mentioned earlier, nuclear polyhedra usually dissolve readily in weak alkali, leaving behind a thin membrane containing a number of virus rods. The behaviour of the nuclear polyhedra of *T. paludosa* towards different reagents is unusual, however, and differentiates them sharply from the other known types of nuclear polyhedra. They are resistant to trypsin, and to dilute and weak acids and alkalis. In molar sodium hydroxide, however, they elongate to several times their normal length, becoming first biconvex spindles and then extending into crescents or worm-like shapes. At about three times their normal length the elongation is still completely reversible, and in water at *p*H 5–8 they return to their original size and shape. The elongation and the return to normal length (which takes place along the same axis) can be repeated indefinitely and as rapidly as the necessary solutions can be alternated. The failure of the polyhedral bodies to dissolve makes it impossible to see by electron microscopy any virus particles contained within. It is necessary to examine thin sections of the polyhedra in order to see what is inside them. In the electron microscope the sections reveal the presence of rod-shaped particles, presumed to be the elementary virus particles. Figure 5 shows some such sections, some apparently longitudinal and some transverse.

Larvae of *T. paludosa* collected from the field are occasionally found to be harbouring a virus disease easily recognizable by the change in appearance that it induces in the body of the affected host [14, 18, 19]. Whereas the normal appearance of these larvae is a dark tan, the colour of the diseased ones is a somewhat opalescent blue-indigo (figure 13). Examination by low-power microscopy shows that the colour is particularly intense in the lobes of the hypertrophied fat-body (figure 14); as the disease approaches its termination the colour is less intense and is more generally dispersed throughout the body. When sections of diseased fat-body tissue are examined in the electron microscope it is seen that the cells are filled, but only in the cytoplasm, with darkly staining particles (figure 6), believed to be the elementary virus particles. There is no indication of encapsulation around the virus particles, nor is there any sign of the development of a polyhedral body, such as would be seen in a cytoplasmic polyhedrosis.

It is quite easy to extract and purify the elementary particles from the diseased insects. Immediately after death has ensued the cadavers are simply placed in water, where they are allowed to stay for a few days while the particles leach out. A brief, low-speed centrifuging is sufficient to remove the gross material, after which a high-speed centrifuging will convert the virus particles into a pellet of fairly pure material. A repetition of the low- and the high-speed centrifuging will result in a preparation that is demonstrably homogeneous and is presumably pure. When this material is fed to healthy larvae there is an incidence of the disease distinctly higher than that found in the wild population: from this observation it may be concluded that the particles in the preparation are the causative agent of the disease. The amount of virus produced by one larva is remarkably great: measurements so far made indicate that in the late stage of the disease about one-fourth of the larva's dry body-weight will have been converted to virus particles. This figure is not approached by any other known animal virus disease, although it is approximated by the tobacco mosaic virus disease and by a few of the virus diseases of bacteria.

Pellets formed from purified TIV are found to have fascinating optical properties. When observed by transmitted light they are generally orange in colour (figure 16), not greatly different from the colour exhibited by pellets of other viruses. But when examined by reflected white light the pellets appear exquisitely iridescent, with the colours most noticeably in the blue and green regions of the spectrum. The predominant colour is related to the degree of hydration of the pellet; very 'wet' pellets are more yellow and orange (figure 17), while 'dry' ones are characteristically violet. The origin of the colour is disclosed by examination of thin sections cut through pellets that have been dehydrated and embedded for sectioning. A typical section will show a somewhat bizarre pattern of particle array, such as is shown in figure 7. As is readily seen, this pattern is what would be expected if a thin slice were made through an array of randomly orientated microcrystals. From this kind of observation it is concluded that the pellet is a mass of small crystals, each crystal being of the order of 10 microns across. In fact, the composition of a pellet can be thought of as a scaled-up model of a polycrystalline metal, such as a piece of cast alloy. It is well known that a specimen of a polycrystalline metal will exhibit Bragg reflections when illuminated with X-rays. The conditions for such reflections are that the interatomic spacings in the crystals be about half the wavelength of the X-rays employed, and that the orientation of a crystal with respect to the incident X-ray beam be such that the scattered, or 'reflected', X-rays experience constructive interference. If we were to build our crystal from large-size virus particles instead of from atoms, and substitute light waves for X-rays, we should have the proper situation for the exhibition of Bragg reflections in the visible region of the spectrum. Since we illuminate with light of all wavelengths (white light), each virus crystal will be oriented in a manner such as to reflect constructively some particular wavelength, and the overall effect will be iridescence—the reflection of light of differing colours by multiple, tiny regions. Pellets exhibiting these optical properties have not been seen for any other virus, owing to failure to satisfy the condition that the optical path-length between adjacent planes in the crystals be as large as half the wavelength of visible light. But the particles of TIV are so large that the inter-particle spacing is about 1500 Å, and, assuming a refractive index of about 1·5, the optical path-length is about 2300 Å. Hence constructive interference should occur with light of blue-green colour. Other animal viruses are known that are even larger than TIV, but they are apparently not sufficiently uniform in size and shape to crystallize.

Purified preparations of TIV have a tendency to crystallize when allowed to remain undisturbed

in the cold, although their behaviour in this respect is not entirely predictable. A suspension of TIV in an ordinary glass tube will generally be found to have crystallized in part, with a mass of small, brilliantly reflecting crystals at the bottom and a diminished concentration of virus in the supernatant fluid (figure 15). On one occasion it was possible to form an array of crystals on the flat surface of a microscope slide, in which position they can readily be photographed (figure 18). Attempts made so far to induce crystallization by the addition of half-saturated ammonium sulphate (the classical method of crystallizing plant viruses) have been only moderately successful. Since it would be of considerable interest to examine such crystals by X-ray and optical analysis, the conditions necessary for their formation, and their stability when formed, must be further investigated.

Since the particles of TIV may be obtained obviously quite pure and in considerable quantity, they are fit subjects for chemical and physical investigations designed to give a better understanding of the structure of viruses. Only preliminary chemical analysis has so far been undertaken, and from this it appears that the virus particles consist of nucleoprotein. The nucleic acid portion is all of the deoxy type and constitutes about 15 per cent of the mass of the virus. A determination of the proportions of basic substances indicates that these are distributed much as in other insect viruses analysed. Preliminary sedimentation studies show that the virus particles form a sediment with a sharp boundary—a result not surprising in view of the high degree of morphological homogeneity, indicated by the evidence of crystallization and by electron microscopy.

The morphology of the particles of TIV may be examined by electron microscopy of thin sections of material containing them and of preparations of purified virus. Micrographs of sections show two characteristic features (figure 8), one expected and one new. The expected observation is that the particles possess to some degree a non-uniform internal structure. There appears to be an outer envelope, within which is a relatively transparent region; the central area of the particle is filled with opaque material, tentatively identified with its nucleic acid portion. Such differentiation of apparent structure is commonly encountered in sections of the larger animal viruses. The unexpected observation is that six-sided contours fairly frequently occur when the virus particles are seen in section. Regular hexagons are not seen, presumably because of distortion of the sectioned material by the microtome knife, but the relation of the direction of the apparent distortion to the known direction of cutting is such as to imply that the true contours are hexagonal.

Purified preparations of TIV appear quite monodisperse in the electron microscope, each virus particle having a diameter of about 1300 Å. Even in dry specimens prepared from a water suspension in the usual way, and hence suffering the distortions brought about by surface tension, the characteristic contour of the particles is six-sided (figure 9) rather than circular. This appearance is unique among the known viruses of comparable size: vaccinia, for example, is brick-shaped and non-uniform in size, while influenza appears quite circular and heterodisperse.

Since the particles of TIV appear to have six sides when seen in contour, it is reasonable to suppose that they are polyhedral in shape. Other examples of polyhedral-shaped virus particles are known [20] among the bacterial and plant viruses. But in no case has it hitherto been possible to arrive at a convincing notion of the exact form of the polyhedron. The difficulty has been mainly due to the smallness of the particles, making a direct determination of the shapes of the polyhedral facets uncertain. An indirect approach is to infer the three-dimensional shape of polyhedra from the shapes of the shadows formed by application of a shadow-casting technique [21]. With the small viruses this method is unreliable, owing to the relative roughness of the surface of the substrate film (cellulose nitrate) upon which the virus particles must be mounted [22].

The particles of TIV are large enough, and their shape is regular enough, to offer some hope that their full polyhedral shape can be determined by the analysis of shadow shapes. A direct observation of the shapes of the surface facets cannot be undertaken, owing to the great electron opacity of such large objects. In order to attempt shadow analysis it is essential to preserve the three-dimensional shape of the virus particles as they are dried for electron microscopy. This preservation is readily and effectively secured by freeze-drying [23]. As can be seen (figure 10), shadows of particles so prepared are distinctive in shape, and their outlines are not obscured by roughness of the substrate film.

Only three kinds of polyhedra will have six-sided contours when the object is lying upon any one of its facets. These are the octahedron, the rhombic dodecahedron, and the icosahedron. But

as can be seen by trials with models, the octahedron will not cast a shadow having five sides, a type of shadow frequently cast by the frozen-dried particles of TIV (figure 10). The decision, then, lies in a choice between the rhombic dodecahedron and the icosahedron. The correct answer may be found by the application of double shadowing, in which two shadows are cast with azimuth angles 60° apart. In this way a single particle can, as it were, be examined from two points of view. A cardboard model of an icosahedron, illuminated by two light sources placed 60° apart relative to the model, is shown in figure 11. These points are to be noted: when the orientation of the model is such that one point of the hexagonal contour is directed toward one shadow, and the adjacent point is directed toward the other shadow, one of the two shadows formed will be five-sided and with a blunt end, while the other is four-sided and is pointed. A rhombic dodecahedron will not cast a pair of similarly shaped shadows when similarly illuminated. In figure 12 are shown two frozen-dried particles of TIV that have been double-shadowed to test the aptness of the icosahedral model. As can be seen, the correlation in shadow shapes is good. We can therefore conclude that the three-dimensional morphology of one virus is now known and that its shape is that of an icosahedron. What bearing this determination will have upon furthering our notions of virus structure remains to be seen.

ACKNOWLEDGMENTS

We wish to express our thanks to Miss S. Vernon-Smith for obtaining several of the electron micrographs shown here, and to Mr S. Frey for the colour photography.

REFERENCES

[1] THOMPSON, C. G. and STEINHAUS, E. A. *Hilgardia*, **19**, 411, 1950.
[2] WYATT, G. R. *J. gen. Physiol.*, **36**, 201, 1952.
[3] BERGOLD, G. H. *Biol. Zbl.*, **63**, 1, 1943.
[4] BERGOLD, G. H. and BRILL, R. *Kolloidzschr.*, **99**, 1, 1942.
[5] MORGAN, C., BERGOLD, G. H., MOORE, D. H., and ROSE, H. M. *J. Biophys. Biochem. Cytol.*, **1**, 187, 1955.
[6] BERGOLD, G. H. *Z. Naturforsch.*, **2b**, 122, 1947.
[7] SMITH, K. M. and XEROS, N. *Parasitology*, **43**, 178, 1953.
[8] SMITH, K. M. and WYCKOFF, R. W. G. *Nature, Lond.*, **166**, 861, 1950.
[9] SMITH, K. M. *J. Biophys. Biochem. Cytol.*, **2**, 301, 1956.
[10] STEINHAUS, E. A., HUGHES, K. M., and WASSER, H. B. *J. Bact.*, **57**, 219, 1949.
[11] SMITH, K. M. and XEROS, N. *Parasitology*, **44**, 400, 1954.
[12] HUGHES, K. M. and THOMPSON, C. G. *J. infect. Dis.*, **89**, 173, 1951.
[13] WASSER, H. B. *J. Bact.*, **64**, 787, 1952.
[14] XEROS, N. *Nature, Lond.*, **174**, 562, 1954.
[15] SMITH, K. M. *Endeavour*, **10**, 194, 1951.
[16] SMITH, K. M. and RIVERS, C. F. *Parasitology*, **46**, 235, 1956.
[17] SMITH, K. M. and XEROS, N. *Nature, Lond.*, **173**, 866, 1954.
[18] SMITH, K. M. *Ibid.*, **175**, 12, 1955.
[19] WILLIAMS, R. C. and SMITH, K. M. *Ibid.*, **179**, 119, 1957.
[20] WILLIAMS, R. C. *Cold Spr. Harb. Symp. quant. Biol.*, **18**, 185, 1953.
[21] WILLIAMS, R. C. and WYCKOFF, R. W. G. *Proc. Soc. exp. Biol. Med., N.Y.*, **58**, 265, 1945.
[22] KAESBERG, P. *Science*, **124**, 626, 1956.
[23] WILLIAMS, R. C. *Exp. Cell Res.*, **4**, 188, 1953.

The early history of the permanent magnet

E. N. DA C. ANDRADE

The history of magnetism has been but little explored, and it is not at all generally known how powerful were the magnets produced more than two centuries ago or what methods were used for their production. It will be a surprise to many, too, that permanent magnets made from powdered iron oxide—supposedly a quite recent invention—were known in the eighteenth century. This article traces the history of permanent magnets from ancient times up to the invention of the Heusler alloys at the beginning of the present century.

The manufacture of permanent magnets is today a flourishing activity, at once a science and an art, especially at Sheffield. This may cause some surprise, since there appears to be a widespread impression that such magnets have, for all important purposes, been superseded by the electromagnet. However, the permanent magnet plays an essential part in high-tension magnetos, in telephone generators, in telephone receivers, in gramophone pick-up units, in moving-coil loudspeakers, in television focus units and other equipment involving deflection of electron beams, in electricity meters, and in thermostats. It is essential also in many less familiar devices, such as permanent-magnet chucks and magnetic separators, designed to remove unwanted ferruginous particles. Much admirable research and much ingenuity have been, and are being, devoted to the special alloys now generally used, to the methods of magnetization, to shape and general design, including that of soft-iron pole-pieces, and to other highly specialized problems. Even in the field of atomic physics the permanent magnet may be found. On account of the constancy of its field, C. D. Ellis used such a magnet for work on β-ray spectra, the strength being changed by currents applied between the experiments. Large permanent magnets have been used in cosmic ray research and in radio-frequency spectroscopy. In a recent book P. R. Bardell [1] gives a table of some applications of permanent magnets notable for the variety it exhibits.

All these uses belong substantially to the present century—in fact the main developments of the permanent magnet as now known are matters of the last thirty years. In the early days of physics, however, the permanent magnet was the subject of much attention and research, especially before the electromagnet was developed and electric current supply became general. In the histories of physics the pioneering work of William Gilbert on magnetism is recorded, but the interest in the permanent magnet of such men as Canton, Aepinus, Coulomb, and Jamin, cited for other achievements, is neglected, and so are many very interesting achievements of less famous men. Some account will here be given of work up to the end of the nineteenth century, which sees the close of an epoch.

The only permanent magnet known to the ancients was the loadstone or lodestone (literally, 'way stone', from its guiding the mariner on his way; *cf.* lodestar). This is a variety of magnetite, Fe_3O_4, which as found in the natural state is magnetic. It received its classical name *magnes* because it was found in Magnesia, a district in Thessaly. Thales of Miletus (*c.* 600 B.C.) is said to have known of its abnormal properties, and its attractive powers are mentioned by various of the great Greek philosophers of the period 400–200 B.C. Later Lucretius, in *De natura rerum* (vi, 1042), records repulsion, without any very precise notion of what was in question—'Sometimes too it happens that the nature of iron is repelled from this stone, being in the habit of flying from and following it in turns.'

The first artificial permanent magnets were iron needles which had been 'touched' by a loadstone. Much has been written as to when and where the process originated. Round about 1200 the French troubadour Guyot de Provins (whom E. Hoppe [2] calls Hugue de Bercy) records in his satirical poem *La Bible* a compass made of such a touched needle supported by a floating straw. Peter Peregrinus in his famous *Epistola ad Sygerum de Foucaucourt militem de magnete*, 1269, wrote that it was known to all who had tried it that an oblong piece of iron which a loadstone had touched turned towards the pole if floated on a piece of

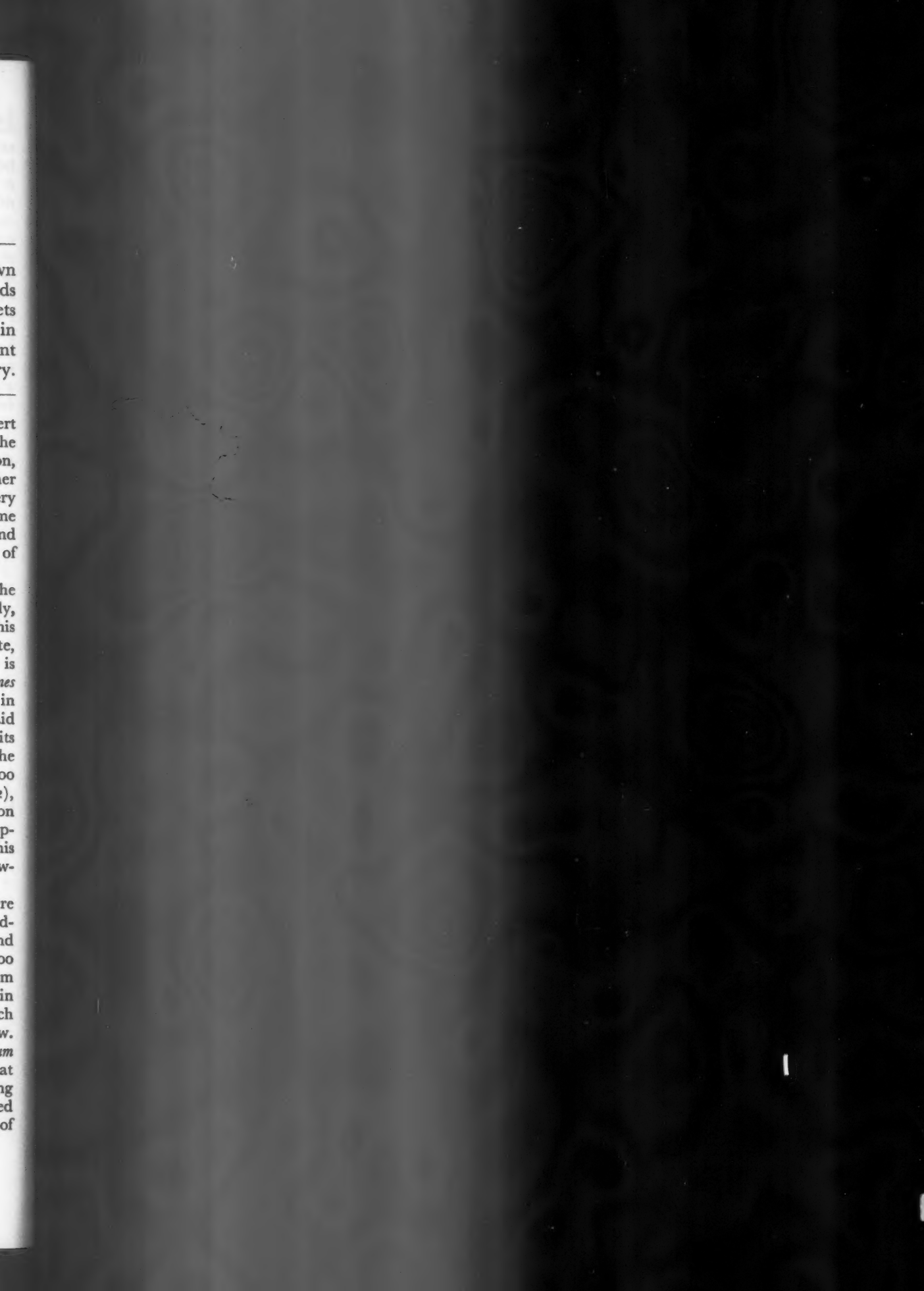


FIGURE 1 – *Gilbert's capped or 'armed' loadstones, as represented in the* De Magnete.

FIGURE 3 – *Smithing an iron rod in the direction of the Earth's field, as shown in Gilbert's* De Magnete. (Septentrio, *north;* auster, *south.*)

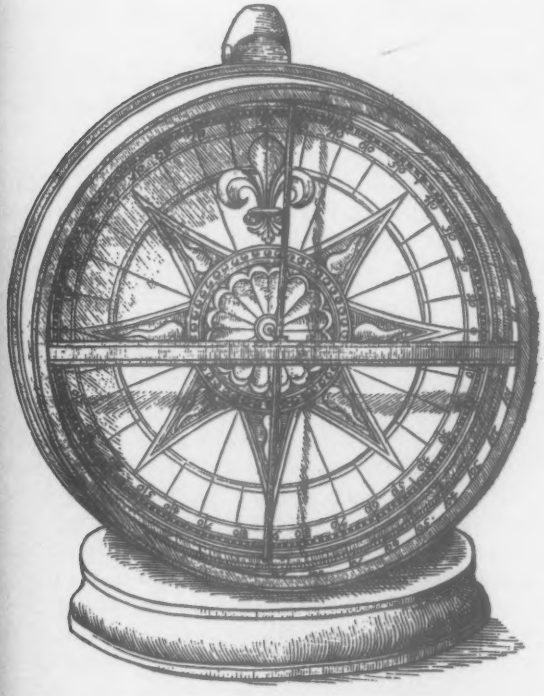


FIGURE 2 – *Gilbert's dip circle, as represented in the* De Magnete.

wood. Robert Norman in his little book 'The Newe Attractive', 1581, likewise speaks of his needles as touched with the stone, but gives no particulars. There are Chinese claims, too, but our concern here is the development of the permanent magnet, as it can be continuously followed, and these few words of reference are intended to do no more than to indicate dates of general knowledge.

The first great systematic work on the magnet is that described by William Gilbert (sometimes spelt Gilberd by contemporaries) in the famous *De Magnete Magneticisque Corporibus et de Magno Magnete Tellure Physiologia Nova*, published in 1600. This is one of the great classics of experimental physics, and its value was at once recognized by, for instance, Galileo, who wrote [3]: 'I extremely praise, admire and envy this author for that a conceit so stupendous should come into his mind, touching a thing handled by infinite sublime wits and hit upon by none of them.' In an age when much that was written about the magnet was worthless tradition and absurd, if amusing, superstition Gilbert stands out as a most careful and critical experimenter. Characteristically, he tells the reader 'Let whosoever would make the same experiments handle the bodies not heedlessly and clumsily but carefully, skilfully and deftly; when an experiment fails he should not ignorantly doubt

our discoveries, for there is naught in these books that has not been investigated and done again and again by us.' An example of the traditional beliefs refuted by Gilbert is that the loadstone lost its attractive power in the presence of, among various other things (including garlic and goats' blood), the diamond: he showed that surrounding the stone with seventy-five diamonds had no effect. Some of his most important work was on terrestrial magnetism, which does not here concern us; but he also carried out fundamental investigations on the magnet itself.

The most powerful magnets of Gilbert's time were still loadstones. He described how to make them more powerful attractors of iron by 'arming' them, that is, by attaching soft iron caps at each end, as shown in figure 1. This is the first example of the soft iron pole-pieces used in many cases with modern permanent magnets. With a particular stone, Gilbert writes that arming increased the weight of iron that could be lifted from 4 ounces to 20 ounces; but he is careful to state that arming does not increase the attraction at a distance.

Gilbert was much concerned with making permanent magnets, or artificial magnets as they were long called to distinguish them from the natural loadstone. He needed these for compasses and dip circles, and called such pivoted needles *versoria*, which we may translate 'turnabouts', the word *versorium* being apparently of his invention. He lays stress on the material to be used: at one place he says that the needles are to be made *ex optimo aciario*, which would seem to mean of the best edge-steel, the word, not classical Latin, being derived from *acies*, an edge, in particular a sword edge (*cf.* French *acier*). Elsewhere he specifies 'the best iron which has been melted down' (*ferrum excoctum*) or 'iron made hard', so that he clearly meant to warn against the use of soft iron. As regards the size of his magnets, he said that his dip circle, shown in figure 2, might be 10–12 inches across, and he speaks elsewhere of a magnet as thick as a goose quill and about 8 inches long.

Gilbert gives three ways by which permanent magnetism (or 'verticity' as he termed it) may be given to steel. The first method was by touch with a single loadstone, which was to be drawn from the middle of the needle to the end, where the application was to be continued by a very gentle rubbing. In discussing the dip needle he speaks of touching the opposite ends with the two different poles of the loadstone, but he never deals with anything but single touch. The second method was forging with the horizontal specimen pointing north and south in the Earth's field, as shown in figure 3. He also recorded that iron wires become magnetic if drawn in a north–south direction, but not if drawn in an east–west direction. This second method, then, was one of considerable plastic deformation in the Earth's field.

Gilbert was quite clear that a red heat destroyed permanent magnetism, but records that a red-hot iron bar left to cool in the direction of the Earth's field became permanently magnetic, and further that unheated iron bars left in the direction of the Earth's field for twenty or more years—such as window bars—likewise acquired verticity. This magnetization by long exposure to the Earth's field without plastic deformation constitutes a third procedure, and these three general methods cover all that were used until the electromagnet was introduced in 1825. If J. Robison's [4] words in 1822, 'It is not saying too much of this work of Dr Gilbert's to affirm, that it contains almost everything that we know about magnetism', were an exaggeration, they were not a very great exaggeration.

Not much advance was made in the seventeenth century. The method of arming loadstones was improved by squaring the ends and applying suitably shaped iron plates, as illustrated in figure 6, which increased the attractive force on iron in contact. A typical mounted loadstone of the seventeenth century is shown in figure 4. Boyle, in 1675, shrewdly observed that the destruction of magnetism by heat without any apparent change in the metal showed that magnetism was a matter of the 'disposition or internal constitution' of the iron, and, in confirmation of Gilbert's general discovery, said that iron fire tongs and such-like that were heated and stood upright—which means nearly in the direction of the Earth's magnetic force in England—to cool sometimes became strongly magnetic. This fact was, as will be seen, applied later.

It must be remembered that loadstones were very expensive. Hartsoeker [5], in 1711, complained that he was asked 3000 guilders for one half as big as his fist, a sum which was then equivalent to about £250, and so to something like £3000 of today. The first man to make strong permanent magnets was Servington Savery [6], who described his processes in 1730. He started with wires about 1/20th inch in diameter and 2¾ inches long, of steel which he 'seasoned very hard'. These he magnetized by touch with a loadstone, but he gives no details as to method. Of 37 such wires he made a hexagonal bundle, like

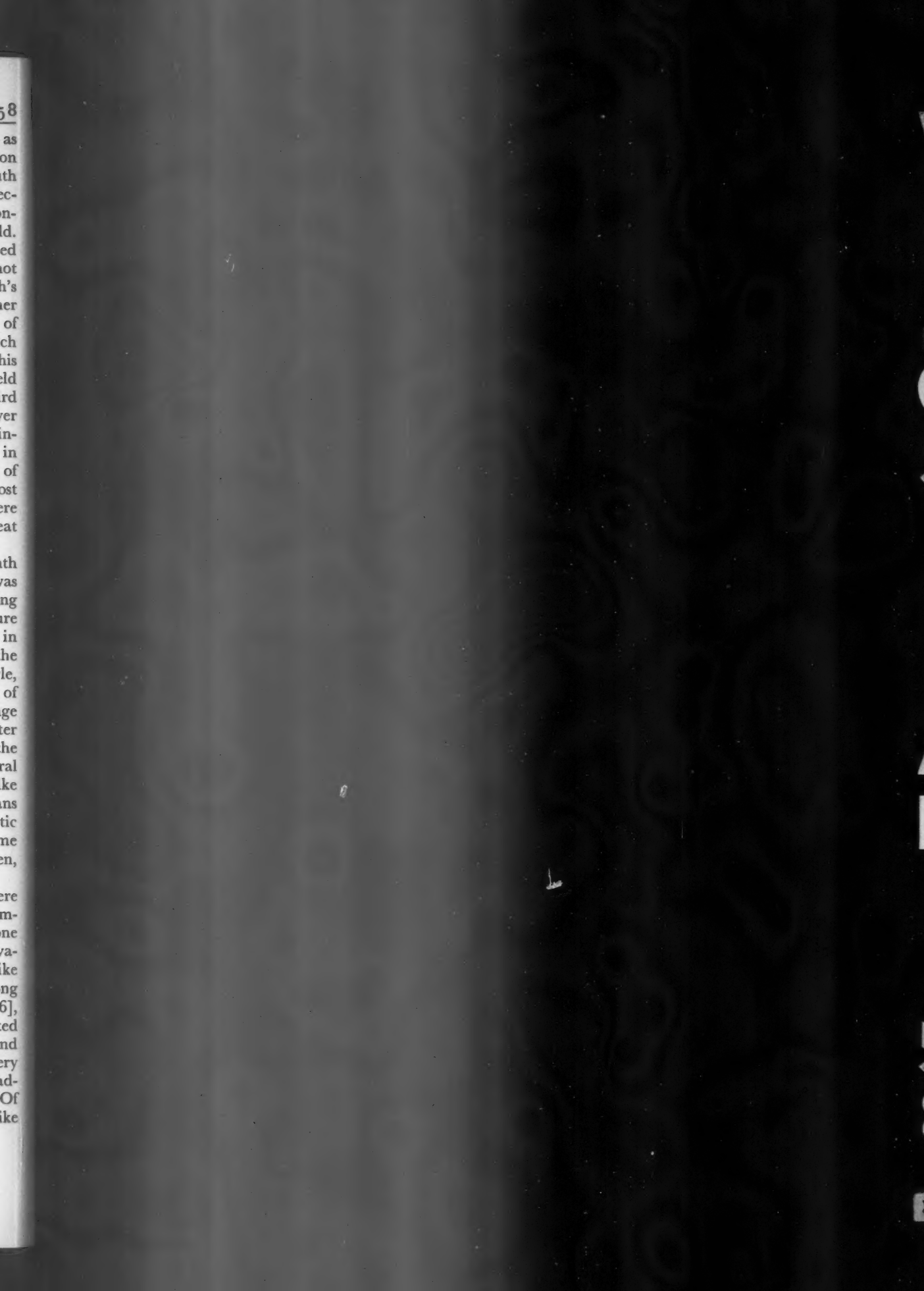


FIGURE 4 – *Stewart silver-mounted loadstone* (circa 1660).

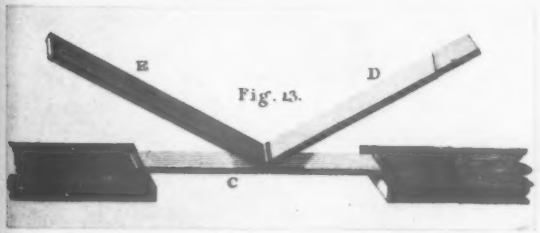


FIGURE 5 – *George Adams's representation of one of Gowin Knight's methods of magnetization.*

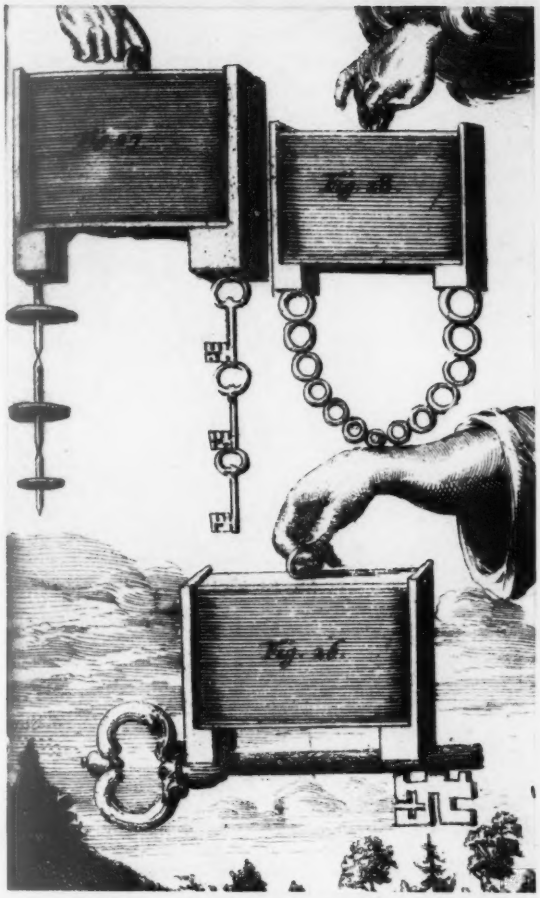


FIGURE 6 – *Armed loadstones as represented by Dalencé in his* Traitté de l'Aiman, 1687.

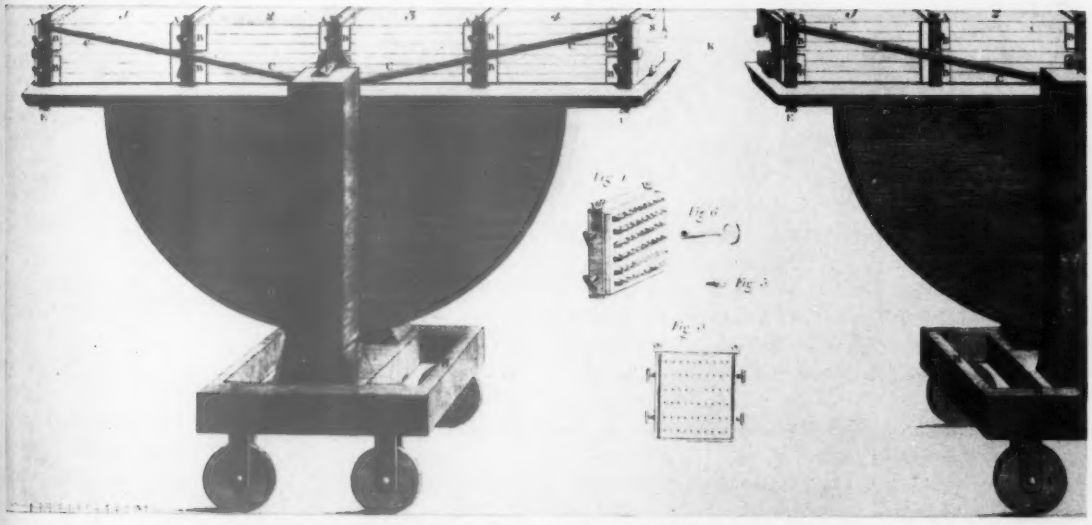


FIGURE 7 – *Fothergill's representation of Knight's magnetic magazine.*

FIGURE 8 – *Adaptation of Knight's magnetic magazine, as used by Faraday.* (Crown copyright, Science Museum, London.)

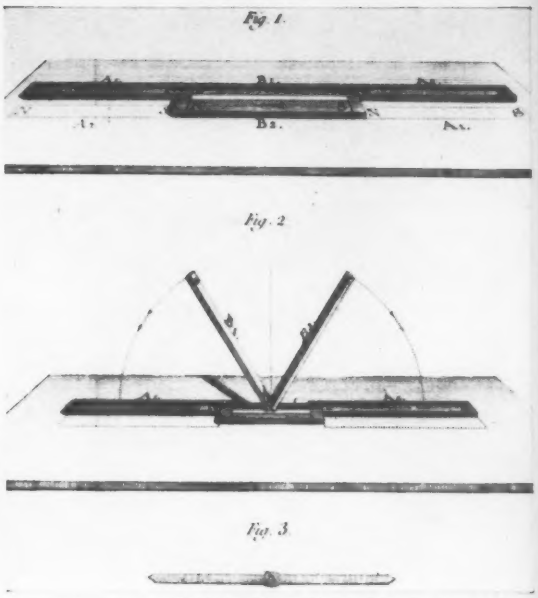


FIGURE 9 – *du Hamel's method of magnetization.*

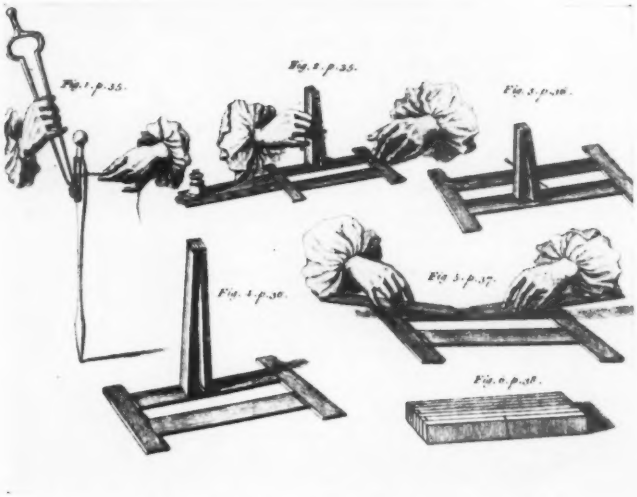


FIGURE 10 – *Canton's methods of magnetizing steel bars.*

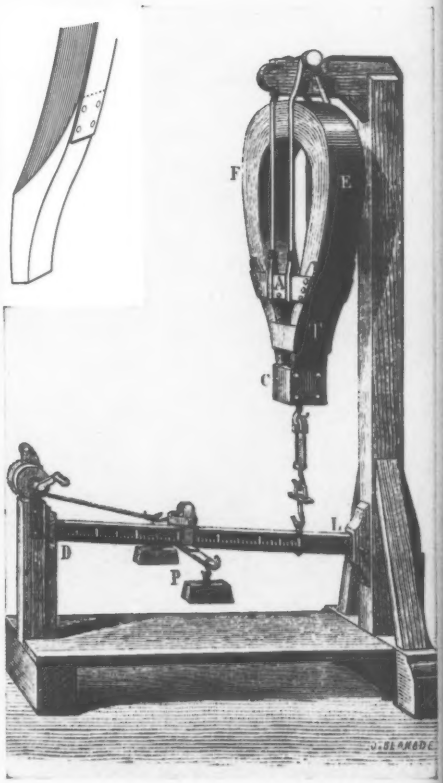


FIGURE 11 (*Right*) – *Jamin's great permanent magnet. The method of fitting the soft iron pole pieces is shown in the inset.*

poles to like, to which he fitted soft iron endpieces or armatures. This was the first compound magnet, made up of smaller ones. With two of these he magnetized steel wires by the method, invented by him, of divided touch, placing the north pole of one magnet and the south pole of the second magnet close together at the middle of the rod to be magnetized and drawing them apart. He also adopted a method, later used by others, of placing several of the stout wires to be magnetized end to end in a groove and treating them as one, rejecting the end ones. A single one of his wires would lift a nail twelve times its own weight. Savery also made, by a complicated method of mutual touches, large bar magnets, 16 × ¾ × ¾ inch, one of which would sustain another when placed end to end. It must be remembered that the lifting power of geometrically similar magnets varies, *ceteris paribus*, approximately as the 2/3rd power of the weight.

Savery was followed by a man who made a European reputation with his magnets, Gowin Knight (1713–72). The spelling of his name gave his French contemporaries much trouble, the extreme version being the Gnith of Sigaud de la Fond: Coulomb calls him Knigth. He carried out a flourishing business—or, to use the language of a politer day, 'procured considerable pecuniary advantages'—with these magnets: Benjamin Wilson [7] speaks of 'two of his best ten guinea bars'. It may be noted that as the first principal librarian at the British Museum his salary was £160 a year.

Knight kept secret many of his methods, including the way in which he magnetized his initial bars. He did, however, communicate a method of divided touch which he used for his compass needles, which were 5 inches long [8]. Two large bar magnets of hardened steel were placed in line, north pole to south, with a small gap between them; the needle was pressed on to the two ends and the magnets were then drawn apart, the process being repeated a few times. George Adams [9] in 1794 described the method illustrated in figure 5, in which the ends of the bar to be magnetized are supported by permanent magnets, and the method of divided touch, with inclined magnets, is employed. The inclined magnets are placed together, north pole to south, as shown, and then drawn apart, the process being repeated a few times and applied to each surface of the bar C. He says that this was one of the methods used by Knight. This George Adams was the son of George Adams the elder, likewise an instrument maker, who was closely associated with Knight in the making of compasses, so that this assertion is likely to be correct. The method is substantially that later used by Coulomb [11].

Knight was particularly renowned for his 'magnetic magazine', a very powerful compound magnet built up of steel bar magnets each 15 × 1 × ½ inch. J. Fothergill [10], who described the magazine after Knight's death, says that these individual magnets were 'impregnated by repeated attritions' but nothing more of the method. Each half of the compound magnet consisted of 240 bars, in four lengths end to end, each being 6 magnets deep and 10 wide (figure 7), so that the approximate overall dimensions were 5 feet 4 inches long by 6 inches by 5 inches. This magnet was presented to the Royal Society by Fothergill, who had inherited it, in 1776. One half had not long before been impaired by fire and replaced by inferior magnets. Later Christie arranged the two halves to form a horseshoe-pattern magnet, which Faraday used for his famous experiment with the rotating copper disk in 1831. Figure 8 is a photograph of the actual magnet as arranged for this experiment, now exhibited in the Science Museum.

As examples of the strength of Knight's magnets, which so impressed his contemporaries, a bar weighing 53 grams lifted 28 times its own weight of iron, one weighing 435 grams lifted 12 times its own weight, while a magnet compounded of 12 bars, weighing in all 620 grams, lifted 14 times its own weight.

In 1938 a distinguished authority, writing in the *Journal of the Institution of Electrical Engineers*, referred to an 'entirely novel' type of permanent magnet prepared in Japan from powdered oxides by Kato and Takei. However, such magnets were made by Gowin Knight in a manner that he kept secret, but which was published in 1779, after his death, by Benjamin Wilson [7]. He put a large quantity of iron filings in a tub of clean water, which he worked backwards and forwards for some hours until he had a suspension of very finely divided iron oxide. He poured off the water and let the particles settle to a sludge. When, by repeating the process, he had sufficient fine oxide powder he mixed it to a stiff paste with linseed oil, which he moulded to desired shapes and baked before a moderate fire. These blocks he then magnetized between the poles of his magnetic magazine and apparently produced very strong magnets in this way. There was formerly one in the British Museum, but I have been unable to trace its fate.

Contemporary with Gowin Knight were John Canton (1718–72) and John Michell (1724–93).

Canton [12] used as his original magnets pokers and tongs that had been heated and left to cool upright. 'The larger they are, and the longer they have been used, the better.' He magnetized small bars of iron by holding them, one by one, to the poker and stroking with the tongs, as shown in figure 10. He then placed two large steel bars parallel to each other, yoked with two shorter soft iron bars, and to effect magnetization stood upright on the steel his small magnets, in pairs, north pole touching south at the top, and slightly separated at the bottom by a large pin. He thus produced an intense field over a short distance in the steel. He then rubbed the grouped magnets up and down the bar. This is the method of double touch. With the magnets so produced he magnetized larger bars by double touch. Finally, he used these magnets to magnetize other hard steel bars by divided touch, as also shown in figure 10: in his words 'the horizontal touch, after the vertical, will make the bars as strong as they can be.' His bar magnets weighing 1¾ ounce would lift sixteen times their own weight of iron.

In the same year (1750) Michell published his 'Treatise on Artificial Magnets', the first book devoted to the subject of making steel magnets. He made his first magnets with a comparatively short loadstone resting with both poles on a steel bar and rubbed to and fro (a variety of double touch), and then used these magnets, three and three, for vertical double touch as described by Canton. Apparently the two had worked out the method independently. He also supported the touching magnets vertically in a wooden frame, yoking them at the top by soft iron (figure 12).

In the middle of the eighteenth century England was the acknowledged home of the permanent magnet, and the highest claim of those in other lands was that their magnets were as good as Knight's. Important work, was, of course, carried out elsewhere, notably in France and Russia. Aepinus [13] used the method of double touch with inclined magnets mounted in a wooden frame which he slid backwards and forwards like a carpenter's plane. du Hamel (also spelt Duhamel) [14] formed a rectangle, as did Canton, but added two stationary magnets, as shown in figure 9, while he used the method of divided touch. Coulomb says that Canton used the method of du Hamel, but although both papers appeared in the same year, 1750, Canton would appear to have priority, for his was read before the Royal Society on 17th January and du Hamel's is dated 8th April. Antheaulme [15] arranged two

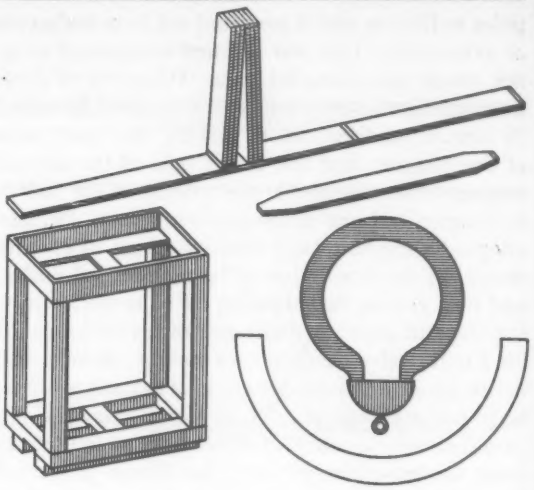


FIGURE 12 – *Michell's illustration of the method of double touch.*

iron bars, each some 15 feet long, in line with a small gap between them, and so inclined to the vertical as to lie in the direction of the Earth's field. At the end of each bar adjacent to the gap was a small projection, or 'heel' (*talon*): the bar to be magnetized was rubbed slowly on these heels. Good results were claimed.

There were many minor differences of methods of touch used by experts in the eighteenth century, but on the whole it is evident, from the figures given, that the best magnets of the period were surprisingly good.

An interesting point about the so-called magnetization by touch, which should be called by rubbing, is the part played by friction. Friction is apparently essential: it is recorded (e.g. by Robison [16]), that oiling the travelling magnets very much impairs the magnetization communicated, as does any other effective lubricant, e.g. talc or gold leaf. Haldat [17] says that steel wires placed in a field so weak as to produce spontaneously but little magnetism acquire marked permanent magnetization if rubbed with any hard body, such as brass, glass, or hard wood. He concludes that any vibration is favourable for magnetization.

Two examples have been given of compound magnets, made up of a number of similar magnets side by side, Servington Savery's small hexagonal bundles and Knight's great magnetic magazine. Thick bars cannot be magnetized all through to saturation by any of the methods of touch, in spite of the instructions given by many of the early

magnetizers to turn the bar over and touch two opposite, or all four, sides. Compound magnets have the advantage that the comparatively thin rods of which they are composed can be magnetized to saturation. However, the individual magnets when placed together with like poles in contact tend to demagnetize one another. Coulomb [18] carefully investigated this matter with assemblies of thin permanent magnets roughly 6 inches long and 1 inch broad and found, for example, that twelve side by side gave a 79 per cent loss as compared to the sum for the same magnets separate. Scoresby [19] in 1832 carried out similar experiments, which led him to make powerful permanent magnets of steel strips separated by air and joined at the ends by soft iron pole-pieces, as represented in figure 13. Such magnets are frequently represented in nineteenth-century textbooks. Nobili, about the same time, improved the compound magnet by diminishing the length of the strips to either side of the central one (figure 14).

With the development of the electromagnet and the decline of the wealthy amateur who, in the eighteenth century, was a customer for scientific curiosities, the study of the permanent magnet was less warmly pursued. There was no industrial demand. However, round about 1873, J. C. Jamin, known to all physicists for his work on optical interference, devoted much attention to the subject [20] and made what he claimed to be the most powerful magnet in the world: it weighed 50 kg and supported a load of 500 kg. His magnets were made of thin sheets of steel—'*les lames minces s'aimantent à cœur.*' In his large horseshoe magnet such magnetized sheets were successively sprung into a horseshoe-shaped sheet of steel screwed to a soft iron armature, as shown in figure 11. The way the sheets, of varying length, were adapted to the armature is shown in the inset. Jamin chose his steel with great care and went systematically into such questions as the relative volume of the pole-pieces. He also made bar magnets of sheets separated by air gaps, as Scoresby had done.

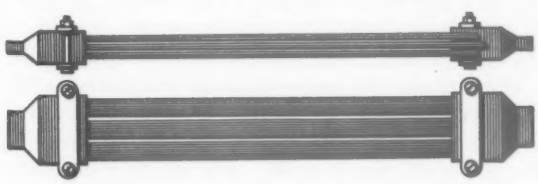


FIGURE 13 – *Scoresby type of permanent magnet.*

FIGURE 14 – *Nobili type of permanent magnet.*

Early work on alloys is of interest in view of the great recent development, for permanent magnets, of magnetic alloys, such as Alnico, containing considerable proportions of metals which, when unalloyed, have feeble magnetic properties. In 1758 Arderon [21] asserted that in certain circumstances brass could be magnetic and decided that the effect was not due to traces of iron. In particular, he found that non-magnetic brass could be converted into magnetic brass by hammering and subsequent double touch. He suggested prophetically: 'Would some ingenious man pursue these experiments perhaps we might have needles made of brass to act as strongly as steel ones.' Similarly Tiberius Cavallo [22], in 1786, recorded that brass could be made magnetic by hammering and the magnetism destroyed by heat, and concluded from experiments in which the 'texture' was altered that 'the property of becoming magnetic in brass by hammering, is rather owing to some particular configuration of its parts, than to the admixture of any iron.' There are many records of brass compass boxes being magnetic, the effect being generally attributed, without any proof, e.g. by Michell, to iron in the zinc.

Sturgeon [23], who made the first electromagnet in 1825, carried out extensive investigations of the magnetic properties of alloys. Having found specimens of silver coinage then in circulation to be magnetic, he pursued experiments which led him to the conclusion that various alloys of copper—with silver, with gold, and with zinc—were magnetic. Careful analysis convinced him that the effect was not due to iron. He showed, for instance, that 1 part of copper to 5 of silver gave a highly magnetic alloy, while 1 part of iron to 20 of antimony gave an alloy practically non-magnetic, as was zinc with a high percentage of iron. This work, published in 1846, showed that the magnetic properties of alloys were not simply determined by the properties of the constituents. Although fundamental for the modern permanent magnet, it seems to have attracted little attention. As regards brass, Sturgeon had been inclined at first to doubt Cavallo's results, but finally states: 'Indeed I am now inclined to embrace the opinion of Cavallo, that the magnetism of brass is not due to the presence of ferruginous material, but depends upon a suitable arrangement of the particles of its proper constituent metals, copper or zinc.'

By 1867 it had been recorded in a German handbook that magnetic alloys could be made of non-magnetic materials and non-magnetic alloys of magnetic materials, particularly iron. The development of magnetic alloys, particularly the study of the effect of adding non-magnetic metals to iron and to nickel, belongs to the present century and so is outside the scope of this brief account. Mention may, however, perhaps be made of the Heusler alloys, since they are over fifty years old, the first publication being in 1901. They are non-ferrous ferromagnetics, and began with a non-magnetic manganese-copper alloy, the addition of aluminium to which produced a striking ferromagnetism. A typical Heusler alloy has the composition Cu_2MnAl, but in general the range of composition is 10–30 per cent manganese and 9–15 per cent aluminium, the remainder being copper. Many of the modern permanent magnet alloys, for instance the various Alnicos and Alcomaxes, contain substantial percentages of aluminium and copper, but contain iron, nickel, and cobalt as well. Their properties and development are outstanding examples of the successful application of accumulated knowledge which the methods of modern metallurgy and applied physics have produced. In contrast, the story which is here attempted is, perhaps, remarkable for the results attained with very simple means when metallurgy as a science was in its infancy and applied physics was represented by a few men of great scientific gifts following their personal predilections.

ACKNOWLEDGMENT

I am very grateful to Professor L. F. Bates, F.R.S., and to Dr Alun Edwards, of the Permanent Magnet Association, for kindly help in connection with certain points in this article.

REFERENCES

[1] BARDELL, P. R. 'Magnetic Materials in the Electrical Industry', p. 73. Macdonald & Co., London. 1955.
[2] HOPPE, E. 'Geschichte der Physik', p. 339, Vieweg, Braunschweig. 1926.
[3] GALILEO. 'Dialogo sopra i due Massimi Sistemi del Mondo'. Dialogo terzo. (Salusbury's translation).
[4] ROBISON, J. 'A System of Mechanical Philosophy', Vol. IV, 209. Edinburgh. 1822.
[5] UFFENBACH, Z. C. VON. 'Merkwürdige Reisen durch Niedersachsen, Holland und Engelland'. Vol. III, 730, 1753.
[6] SAVERY, SERVINGTON. *Phil. Trans. roy. Soc.*, **36**, 295, 1730.
[7] WILSON, BENJAMIN. *Ibid.*, **69**, 51, 1779.
[8] KNIGHT, GOWIN. *Ibid.*, **43**, 161, 1744; **44**, 656, 1747.
[9] ADAMS, GEORGE. 'Lectures on Natural and Experimental Philosophy', Vol. IV, 449, 1794.
[10] FOTHERGILL, J. *Phil. Trans. roy. Soc.*, **66**, 591, 1776.
[11] COULOMB, C. A. *Mém. math. phys. Acad. R.*, **9**, 165, 1780.
[12] CANTON, J. *Phil. Trans. roy. Soc.*, **47**, 31, 1750.
[13] AEPINUS, F. U. T. *Acta Acad. Moguntinae*, **2**, 255, 1759.
[14] DU HAMEL, *Mém. Acad. R. Sci.*, 154, 1750.
[15] LALANDE, *Ibid.*, 211, 1761.
[16] ROBISON, J. 'Encyclopaedia Britannica' (fourth edition), Vol. XII, 375, Edinburgh. 1810. 'A System of Mechanical Philosophy', IV, 283, Edinburgh. 1822.
[17] HALDAT. *Ann. Chim. Phys.*, **42**, 33, 1829.
[18] COULOMB, C. A. *Mém. prés. Acad. Sci., Paris*, 455, 1789.
[19] SCORESBY, W. 'Magnetical Investigations', Vol. I, London. 1844.
[20] JAMIN, J. C. Various papers in *C. R. Acad. Sci., Paris*, 1872–76; 'Cours de Physique' (third edition), Vol. IV, 326, 1883.
[21] ARDERON, W. *Phil. Trans. roy. Soc.*, **50**, 774, 1758.
[22] CAVALLO, T. *Ibid.*, **76**, 62, 1786; **77**, 6, 1787; 'Treatise on Magnetism', p. 283, London. 1787.
[23] STURGEON, W. *Mem. Manchr. lit. phil. Soc.*, **7**, 625, 1846.

The hydration of ions in solution

R. P. BELL

Aqueous solutions of ionized substances are of exceptional importance and there is a vast literature describing and discussing their properties. It has long been assumed that in such solutions ions may be closely linked with one or more molecules of water, but measurements by different methods of the degree of hydration long gave very conflicting results. More recently, however—and with a clearer picture in mind of the structure of water itself—much more consistent values have been obtained, making possible some useful generalizations.

Liquids are less perfectly understood than either of the other two states of aggregation, because they possess neither the well-defined geometrical arrangement of solids nor the freedom from intermolecular action of gases. This generalization applies even to liquids composed of essentially spherical atoms, such as those of mercury and the rare gases, and even more to common solvents which consist of less symmetrical molecules. It might therefore be thought that liquid mixtures and solutions would present a still more intractable problem, but in fact certain properties of solutions—the so-called 'colligative' properties, which depend upon the number of molecules and not their nature—can be treated without any assumption of knowledge of the interactions between solute and solvent. The laws of dilute solutions are well known: though solutions of electrolytes do not obey these laws exactly, even at high dilutions, they can be modified to take into account the electrostatic forces between the ions. The theory of interionic attraction is rightly regarded as one of the great successes of physical chemistry: in fact, its success sometimes obscures our ignorance of other fields of solution chemistry.

Only rarely can we ignore solute-solvent interactions, which are of primary importance in determining many of the individual properties of solutions, such as density and viscosity. These interactions are maximal when the solute is ionic, and the simplest picture of such solutions involves solvation, i.e. the definite attachment of solvent molecules to the ion. Most of our knowledge of solvation relates to aqueous solutions. This is perhaps unfortunate from a theoretical point of view, because, as we shall see, the nature of liquid water itself is still not fully understood. However, the importance of the problem is obvious, since ionic hydration influences all equilibria and transport problems involving ions, and thus has a bearing on a wide variety of industrial, laboratory, and biological processes.

From the earliest days of the ionic theory it has been suggested that in aqueous solutions ions are hydrated, the idea probably first arising from observation of the frequent occurrence of water of crystallization in solid salts. That water molecules should become attached to an ion is reasonable from a molecular point of view. Since the charge distribution in a water molecule can be represented as $^{\delta+}H\!-\!O^{\delta-}\!-\!H^{\delta+}$, cationic and anionic hydration can be represented by $X^+ \ldots O(H)H$ and $Y^- \ldots H\!-\!O\!-\!H$ respectively.[1]

Unfortunately the concept of hydration was often used with more enthusiasm than discrimination, and little attempt was made to define clearly what it implied. In particular, hydration was frequently invoked simply as a convenient means of accounting for discrepancies between observed properties and those predicted by some theory of solution of doubtful validity, rather as 'association' was used to explain discrepancies in the behaviour of solutions of non-electrolytes. This led to the kind of situation described in 1949 by J. O'M. Bockris [1], who pointed out that different methods and authors had produced the following figures for the hydration number of the sodium ion: 1, 3, 4, 17, 71, and (approximately) 700. It is not surprising that there was much dissension on the subject [2], and that the whole concept of hydration in solution fell into disrepute.

[1] Hydration of cations is sometimes considered to involve a co-ordinate link, but it seems probable that an electrostatic formulation is closer to the truth: in any case the distinction between the two pictures is not a sharp one.

In the period 1920–40 the great successes of the interionic attraction theory in accounting for the properties of very dilute solutions of electrolytes produced a considerable prejudice in favour of 'physical' rather than 'chemical' theories of solution, solvation being placed in the latter class. However, it has become increasingly evident that consideration of the immediate surroundings of ions in terms of electrostatic theory is not really profitable. For example, Born's well-known expression [3] gives the electrostatic energy of an ion in a medium of dielectric constant D as e^2/Dr (where e = charge and r = ionic radius). If this is to be applied logically, however, it is quite incorrect to use the ordinary macroscopic value of D, since the solvent in the immediate neighbourhood of the ion is subject to enormous electrostatic fields and hydrostatic pressures, which will greatly modify the effective value of D. No exact theory of these effects is available, and the 'effective dielectric constant' thus approaches the status of an indefinitely adjustable parameter. Under these conditions 'physical' treatment has no advantage over 'chemical' treatment as a means of representing the interaction between the ion and the solvent. This difficulty does not arise when considering the variation with concentration of properties of dilute solutions, as in the successful applications of the interionic theory. It does, however, affect any attempt to account for the properties of different salts or of solutions in different solvents, and also any treatment of concentrated solutions.

During the last ten or fifteen years there has been a renewed interest in the solvation of ions, especially in aqueous solutions. This has been partly because of the difficulties of the electrostatic treatment, outlined above, but also because modern methods of investigation have provided a clearer understanding of the nature of liquid water, and of the way in which it can be modified by the presence of ions. In particular, it is recognized that although an ion can have some effect on water molecules at a relatively large distance, molecules in its immediate neighbourhood will be bound particularly firmly: consequently it is often legitimate to distinguish between primary and secondary hydration. The concept of a primary hydration number is useful only to the extent that a single number will account for a variety of properties of a given ion. All the molecules concerned will contribute to the effect of hydration on different properties, but the contribution of each will depend upon the particular property in question. In the following paragraphs some account will be given of the main properties affected by hydration, with special emphasis on primary hydration. Some estimated hydration numbers are given at the end of the article.

MEASUREMENT OF HYDRATION OF IONS

One of the oldest methods of estimating ionic hydration is based upon the transport of water that occurs when a current passes through a solution. A classical method of detecting this transportation [4] is to add to the solution a non-electrolyte, which is assumed not to be transported by the current. The change in the concentration of this reference substance then gives a measure of the net transport, i.e. the difference between the amounts of water transported by the anions and the cations. Unfortunately there is now a good deal of evidence [5] that non-electrolytes of the type usually employed are also transported with the ions: in fact, it seems likely that any suitable non-electrolyte will owe its solubility to the presence of polar groups, and will therefore become attached to ions in the same way as water itself. Even if this difficulty could be overcome, the method still has the inherent disadvantage that it gives only the difference between the hydration numbers of cation and anion and not absolute values.

A somewhat similar, but more satisfactory, approach involves the interpretation of diffusion coefficients of electrolytes in concentrated solutions. This method is of recent date, and depends upon great advances both in the theory of diffusion and in experimental methods. The detailed theory is complicated [6], but it is easy to see qualitatively that the transport of water will affect the observed diffusion coefficient in concentrated solutions but not in dilute ones, so that information about the water transport can be obtained from accurate measurements over a large concentration range. Since both ions move in the same direction this method gives values for the sum of the two hydration numbers, and certain assumptions are needed to obtain the individual values. It seems likely that only the first layer of water molecules will be bound firmly enough to move as a unit with the ion, so that primary hydration numbers should be obtained.

Many attempts have been made to interpret ionic mobilities in terms of solvation, and methods entailing measurement of these have the great advantage that we obtain quantities referring to individual cations and anions, rather than to the

sums or differences. It was pointed out as long ago as 1915 [7] that the mobilities of the alkali cations ($Li^+ = 38{\cdot}7$, $Na^+ = 50{\cdot}1$, $K^+ = 73{\cdot}5$, $Rb^+ = 77{\cdot}8$) increase as the ionic radius increases, which is the opposite of what one would expect. This can clearly be accounted for qualitatively by assuming that the hydration of the cation decreases along the series, but it is difficult to interpret this quantitatively in terms of a definite hydration number. There is considerable doubt about the absolute hydration numbers derived by this method, but they have reasonable values and show the same trends as those derived by other methods.

The methods so far described relate to various transport phenomena, but there are thermodynamic methods based on the effect of hydration upon the equilibrium properties of ionic solutions. These have the advantage that the underlying theory is better understood than that of transport phenomena: on the other hand, the latter may give information about the interactions in the immediate neighbourhood of an ion, whereas study of any thermodynamic quantity necessarily gives an overall measure of the mutual interaction of all parts of the system. The most direct thermodynamic method derives from the salting-out of non-electrolytes by added electrolytes. If water is attached to the ions there will be less free water to dissolve the non-electrolyte, so that the extent of hydration can be calculated from the observed solubility decrease. This type of argument has been extensively used, but unfortunately the results obtained vary according to the non-electrolyte chosen, and there seems no doubt that this simple picture of salting-out is misleading [8].

A more fruitful approach concerns the properties of the electrolyte solution itself. The use of colligative properties, especially freezing point depressions, is a very old one, many of the early estimates of hydration of solutes being based on observed deviations from Raoult's Law. In a concentrated solution the hydration of solute particles can appreciably decrease the molar fraction of free water, thus leading to such deviations. However, the earlier treatments of this problem neglected all other reasons for departure from ideal behaviour, in particular the ionic forces. In order to avoid the absurdity of large negative hydration numbers for dilute solutions it was necessary to introduce spurious 'degrees of dissociation', usually derived from conductivity measurements, so that little meaning can be attached to these early values. N. Bjerrum [9] was the first to relate interionic forces and hydration. The same idea has appeared again recently in the work of R. H. Stokes and R. A. Robinson [10], and of E. Glueckauf [11] in their interpretations of the activity coefficients of electrolytes (mostly derived from measurements of freezing points or vapour pressures) over a large range of concentrations.

Both treatments allow for the effect of interionic attraction by the standard expression first derived by Debye and Hückel. All deviations from this expression are attributed to the removal of free water by hydration of the ions, the detailed interpretation of these deviations differing in the two treatments. It is found possible to reproduce the observed activity coefficients by assuming for each ion a fixed hydration number that is independent of concentration, and (in Glueckauf's treatment) independent of the ion of opposite charge with which it is associated. The main criticism of this treatment is that the hydration term must really depend upon a number of other factors, such as ion-pair formation and variation of dielectric constant. However, there seems no doubt that hydration is of major importance in concentrated solutions. It has similarly been shown recently [12] that the rapid rise of the acidity function of strong acids with concentration can be mainly attributed to the hydration of the hydrogen ion beyond the stage H_3O^+.

Since water molecules attached to an ion will have only a limited freedom of movement, it is natural to examine ionic entropies in solution as a source of information about hydration. Experimental data lead directly to values for the entropies of dissolved salts, but it is difficult to split these values up into the separate contributions for anions and cations: a number of different methods have been devised, leading to somewhat different final values [13]. Fortunately the uncertainties thus introduced are not great, and in any case do not affect the relative entropy values for a series of anions or cations. These relative values show trends which can reasonably be interpreted in terms of hydration. For example, the entropy becomes more negative with increasing ionic charge, and also with decreasing radius, as illustrated by the following sequences of solution entropies,

$Na^+ +14$, $Mg^{2+} -32$, $Al^{3+} -76$;

$Mg^{2+} -32$, $Ca^{2+} -11$, $Sr^{2+} -7$, $Ba^{2+} +2$;

$F^- -2$, $Cl^- +13$, $Br^- +20$, $I^- +25$.

However, great difficulties arise when we attempt to interpret these figures quantitatively in terms of ionic hydration. This is most strikingly shown by the fact that the solution of two gram-atoms of argon actually involves an appreciably greater loss of entropy (31 e.u.) than the solution of gaseous K^+ and Cl^- to give aqueous potassium chloride (23 e.u.). Thus while both solution processes lead to a state of greater order, the result of replacing two uncharged argon atoms by the ions K^+ and Cl^- is to decrease the ordering effect, which is just the opposite of what would be predicted on the hydration theory.[1]

There are many similar anomalies, and the problem is far from being solved, even qualitatively. There is no doubt, however, that the root of the problem lies in the associated structure of water itself, which we shall now consider briefly.

THE STRUCTURE OF WATER IN RELATION TO IONIC HYDRATION

If we compare the physical properties of water with those of closely related hydrides, e.g. NH_3, H_2S, HCl, we find that its boiling point, heat of vaporization, entropy of vaporization, and dielectric constant are all unexpectedly high. These, and many other anomalies, can be explained qualitatively by the assumption that liquid water does not consist of single H_2O molecules, but is highly associated. This has been realized for a long time, and the older picture described water as a mixture of molecules H_2O, $(H_2O)_2$, $(H_2O)_4$, etc., the proportions of which were affected by temperature and by the addition of salts. Although this idea has been recently revived by A. Eucken and his collaborators [14], the current picture of association is a rather different one. J. D. Bernal and R. H. Fowler [15] pointed out in 1933 that the X-ray diffraction patterns given by liquid water closely resemble those of ice, which is known to have an open tetrahedral structure held together by hydrogen bonds, and they suggested that the liquid has a similar quasi-crystalline structure. Later work has suggested that the structure of the liquid either involves smaller units or is more flexible than originally supposed [16], but there is no doubt that some degree of order is present, and will be affected by solute molecules.

The presence of many uncharged solute molecules helps to stabilize the open structure, thus decreasing the entropy. For an ion we must imagine an inner zone, in which the water molecules are strongly orientated by the charge, surrounded by a region of disorder in which the incompatible ordering effects of the ion and of the tetrahedral water structure largely annul one another. This complex problem has been considered in detail by many authors, notably H. S. Frank and M. W. Evans [17], but it cannot be said that entropy data can yet be related simply to primary hydration, which is only one of the factors involved. The same is true of the effect of ions on the specific heat of water, which, like the entropy, is too sensitive a property to be related simply to a hydration number.

A more useful line of approach is through consideration of the effect of ions on the volume of water, i.e. from the apparent volumes of ions in solution. These apparent volumes are always less than those in the crystal, and may even be negative for multiply charged ions: this is clearly because the normal open structure of water is replaced by a more closely packed arrangement of water molecules round the ion. However, it is difficult to estimate the volume decrease thus produced, and to derive absolute hydration numbers from it.

The compressibility of water is also decreased by the presence of ions, and the simplest interpretation of the facts is that the water in the primary hydration shell is already compressed to the maximum extent, the remainder having its normal compressibility. On this assumption hydration numbers are readily calculated, and some of the results are given in the table at the end of this article. The use of this method has been stimulated by the development of ultrasonic methods for measuring compressibilities of liquids [18].

CONCLUSION

There are many other methods for studying the hydration of ions, and the literature is vast. It will be realized that the concept of a 'hydration number' is not a very well defined one, and that different methods will give different kinds of average values. Nevertheless, if we confine ourselves to properties expected to depend mainly on primary hydration, there is some measure of agreement between different methods, as is shown by the following table. Different authors have given somewhat different values, and the figures in the table have for present purposes been rounded off to the nearest whole number.

[1] The numerical values of the entropies of solution depend on the standard states chosen. The figures given here correspond to concentrations of one mole per litre both in the gas phase and in solution.

Estimated hydration numbers

Ion	Method			
	Mobilities	Diffusion	Activities	Compressibilities
Li^+	5	3	3	4
Na^+	4	1	2	5
K^+	4	1	1	5
Mg^{2+}	12	—	5	12
Ca^{2+}	10	—	4	10
La^{3+}	13	—	8	18[1]
F^-	—	—	2	4
Cl^-	4	0	1	2
Br^-	2	1	1	1
I^-	1	1	1	1

[1] Value for Pr^{3+}.

The agreement between the numbers in this table is far from perfect, but it constitutes a great improvement on the kind of figures quoted at the beginning of this article, and the actual values are reasonable. Examination of these values, and many more, leads to the following generalizations:

(i) the hydration number of cations increases with increasing charge and decreasing size,

(ii) with the exception of the fluoride ion, anions are less hydrated than cations.

There are a number of modern techniques which can give special information about the state of water in the neighbourhood of dissolved ions, though so far they have been applied in isolated cases. Thus isotope exchange using ^{18}O has shown that the water molecules attached to chromic ions in solution exchange only slowly with the rest of the solvent, and the method fixed the hydration number of this ion as 6. With other ions investigated the exchange was too fast to measure, but measurements on exchange equilibria gave some information about hydration [19]. Similarly, proton magnetic resonance gives information about the electrical environment of the protons in the water molecule, and about the way in which this is changed by the presence of dissolved ions [20]. Finally, the fine structure of the absorption spectra of the ions of transition and lanthanide elements depends upon the geometrical symmetry of their surroundings: this has so far been investigated mostly in crystals, but in a few instances it has been used to draw conclusions about the arrangement of water molecules round an ion in solution [21]. The last two methods obviously hold great promise for future investigation.

To sum up, it may be said that the concept of ionic hydration in solution is necessary in order to account for the individual properties of dissolved ions, and also for the behaviour of concentrated electrolyte solutions in general. The idea of a fixed primary hydration number is a useful first approximation in dealing with a number of properties, but a complete understanding of the problem must take into account the peculiar associated structure of water, and the effect of ions upon it even outside the primary hydration sphere.

REFERENCES

[1] Bockris, J. O'M. *Quart. Rev. chem. Soc., Lond.*, **3**, 179, 1949.
[2] Discussion in *Trans. Faraday Soc.*, **3**, 1907.
[3] Born, M. *Z. Physik*, **1**, 45, 221, 1920.
[4] Washburn, E. W. *J. Amer. chem. Soc.*, **31**, 322, 1909.
[5] Longsworth, L. G. *Ibid.*, **69**, 1288, 1947.
[6] Robinson, R. A. and Stokes, R. H. 'Electrolyte Solutions'. Pp. 293–330. Butterworths Scientific Publications, London. 1955.
[7] Remy, H. *Z. phys. Chem.*, **89**, 469, 1915.
[8] Debye, P. and McAulay, J. *Ibid.*, **26**, 23, 1925.
[9] Bjerrum, N. *Z. anorg. Chem.*, **109**, 275, 1920.
[10] Stokes, R. H. and Robinson, R. A. *J. Amer. chem. Soc.*, **70**, 1870, 1948.
[11] Glueckauf, E. *Trans. Faraday Soc.*, **51**, 1235, 1955.
[12] Wyatt, P. A. H. and Bell, R. P. *Faraday Soc. Discussions*, **24**, 1957.
[13] Gurney, R. W. 'Ionic Processes in Solution'. Pp. 172–86. McGraw Hill Publishing Co. Ltd., London. 1953.
[14] Eucken, A. *Z. Elektrochem.*, **52**, 255, 1948.
[15] Bernal, J. D. and Fowler, R. H. *J. chem. Phys.*, **1**, 515, 1933.
[16] Feates, F. S. and Ives, D. J. G. *J. chem. Soc.*, 2798, 1956.
[17] Frank, H. S. and Evans, M. W. *J. chem. Phys.*, **13**, 507, 1945.
[18] Barnartt, S. *Quart. Rev. chem. Soc., Lond.*, **7**, 84, 1953.
[19] Taube, H. *J. phys. Chem.*, **58**, 523, 1954.
[20] Shoolery, J. N. and Alder, B. J. *J. chem. Phys.*, **23**, 804, 1955.
[21] Sayre, E. V., Miller, D. G., and Freed, S. *Ibid.*, **25**, 109, 1957.

Le Palais de la Découverte

The *Palais de la Découverte* in Paris, a unique institution for the exposition of science, this year celebrates its twentieth anniversary. Like a number of other French museums, it owes its origin to an international exhibition, in this instance the International Exhibition of 1937. The *Palais* was set up as an adjunct to the exhibition as a means of showing visitors, in a spectacular yet strictly accurate way, some of the fundamental discoveries of modern science. Its success exceeded all expectations, for within the space of six months there were more than two million visitors: at times the crowds were so great that the doors had to be closed. As a result it was decided to make the *Palais de la Découverte* permanent, and it reopened in June 1938. Originally placed under the Minister of Education, it is a part of the University of Paris, and is administered by a council of twenty, of which the Rector is president. It has since become not only an important feature of the scientific life of France but has attained an international reputation. It has staged many special exhibitions of material lent from abroad—such as the penicillin exhibition of 1946 and the Chagas exhibition of 1955—and has also on many occasions lent its own material to the principal scientific museums of the world.

The great importance to science of general understanding of its aims and methods needs no emphasis. There are many means of seeking to achieve this aim—journals, books, radio, and television, to mention but a few of the most important—but among them museums and exhibitions occupy a special place, for they alone give the visitor an opportunity of actually seeing, and perhaps even of handling and using, scientific material and apparatus. Descriptions and pictures can be very informative but cannot have the same reality as the object itself. The fifty-three galleries of the *Palais* contain a wealth of striking exhibits representative not only of the principal branches of science and mathematics but of medicine and surgery. Models and demonstrations are well supported by descriptions, diagrams, and photographs, and to a limited extent films: colour is freely used. There is throughout a most skilful blending of artistry and science, and this is doubtless in large measure a reflection of the interests of the Director. Auxiliary exhibits are used to clarify particular points. A special feature is the number of trained attendants available to answer visitors' questions.

In 1952 a planetarium was added, and this is one of the finest in Europe. The importance of such equipment for stimulating wide interest in astronomical problems—and thence in science generally—is very great, although all too little appreciated. In this connection it may perhaps be remarked in passing that the first planetarium in the United Kingdom is now nearing completion and will be opened in London later this year.

The exceptional interest aroused by the planetarium is well sustained by the astronomical section. One entire room is devoted to the stellar universe. Enormous photographs and many models illustrate the principal features of the universe, and a special section is devoted to the sun. The moon, too, is accorded special attention, and recent events have of course greatly stimulated interest in this satellite. There is a great lunar globe, built on a scale of 1 mm per km, a remarkable reconstruction of a lunar landscape, and a model which demonstrates how eclipses occur.

The *Palais* caters for visitors of three different kinds. Firstly, members of the general public who are interested in scientific matters. Secondly, organized cultural groups. Thirdly, schoolchildren taken round by their teachers. This last group is a large and enthusiastic one, and it is not unusual for schools to make visits ten or fifteen times a year. For them, the experimental demonstrations —for example, of the wave theory of light—are of great importance, for many of these are ones which are beyond the resources of the ordinary teaching laboratory. Some four hundred experiments are conducted daily, and a further two hundred can be arranged at a week's notice.

In brief, the *Palais de la Découverte* is a generously conceived machine for the popularization of science, a point of union between the laboratory and the public, and an institution for scientific teaching at a high level. It is above all a living museum—representative of fields of science which are of current importance and still developing—rather than a collection of exhibits of things which have had their day and are now of only historical interest. There is, nevertheless, a small section devoted to the history of science: this usefully emphasizes the continuity of the subject and helps to set the modern exhibits in perspective. After twenty years, the first disrupted by war, the *Palais* has achieved much and is seeking to achieve still more. Its organization is not static but seeks to keep pace with the tremendously rapid advancement of science. It is a task in which all will wish it success.

Water repellency

N. K. ADAM

Water will collect in droplets on ordinary smooth surfaces if the contact angle exceeds 100°, but the largest contact angles recorded for smooth surfaces do not greatly exceed 105°. As almost all surfaces slowly soak up water, so diminishing the contact angle, their water-repelling properties are not permanent. Roughening of the surfaces, however, may increase the contact angle, and values as great as 160° have been recorded for certain surfaces containing regularly spaced air gaps. This effect has important practical consequences.

The power of altering any surface to make it water-repellent is of great practical value in many ways. Tents and raincoats are obvious examples of the necessity of surface waterproofing. The vast mineral flotation industry provides another kind of example. In the froth-flotation process small quantities of 'collectors' are added to a slurry of ground ore in water; this results in the formation of a water-repellent film on the surface of the particles of the valuable constituents of the ore, so that these attach themselves to small air bubbles blown into the slurry and rise with the bubbles to the surface. A rather less well known instance is 'dropwise condensation'. If, in a condenser, the cooled surface exposed to the steam is clean, condensation occurs as a continuous film, which offers much more resistance to the passage of heat than would the metal surface alone. The efficiency of a condenser can be increased if a water-repellent film can be maintained on the condensing surface, so that the steam condenses as discontinuous drops, leaving most of the surface free from the water film. Water repellency is also of great biological importance: thus the survival of many plants and animals depends upon their ability to shed water from their surfaces.

For smooth surfaces the theory of water repellency is well known. If the angle of contact between the liquid-air surface and the solid is θ, and γ_S, γ_L, and γ_{SL} are the surface tensions of the solid, liquid, and the solid–liquid interface:

$$\gamma_S = \gamma_{SL} + \gamma_L \cos\theta \qquad \ldots.(1)$$

Dupré's equation relates the work of adhesion W_{SL}, i.e. the work required per unit area to separate the liquid from the solid, to the surface tensions:

$$W_{SL} = \gamma_S + \gamma_L - \gamma_{SL} \qquad \ldots.(2)$$

From (1) and (2) it follows that

$$W_{SL} = \gamma_L(1 + \cos\theta) \qquad \ldots.(3)$$

This may be called Young's equation, since it was given, in words and without proof, in Thomas Young's 'Essay on the Cohesion of Fluids', published in 1805 [1]. This equation shows that the contact angle θ is determined by the relative magnitudes of the adhesion of the liquid for the solid, W_{SL}, and the self-cohesion of the liquid, $2\gamma_L$. For a given value of the surface tension of the liquid, the contact angle increases as the adhesion between liquid and solid decreases. An angle of 180° would indicate zero adhesion, but if there is contact between a liquid and a solid there is always some adhesion, so that angles as large as 180° are never found in practice. Equation (3), it may be remarked, is far more useful than (1), since the latter contains the two solid surface tensions γ_S and γ_{SL}, which cannot be measured with any accuracy.

Water repellency is greatest when the contact angle is as large as possible. If it is over 100°, the liquid assumes the form of separate droplets which run over the surface fairly easily and are not difficult to shake off. The largest contact angles so far recorded with water on smooth surfaces are between 105° and 110°; such angles are obtained when the outermost layer of the surface consists almost entirely of methyl groups. Paraffin wax shows this angle. Polyethylene [2] gives a rather smaller angle (94°). The difference between the two substances is usually ascribed to the surface of the wax consisting of CH_3— groups, while the surface of polyethylene, effectively a paraffin of enormously high molecular weight, probably consists of little but methylene, —CH_2—, groups. The largest recorded contact angle between water and a smooth surface is 110°; this was for a carefully purified crystal of hexatriacontane [2]. This angle indicates the adhesion to water to be 48 ergs per square centimetre, almost exactly the same as that between a liquid paraffin and water.

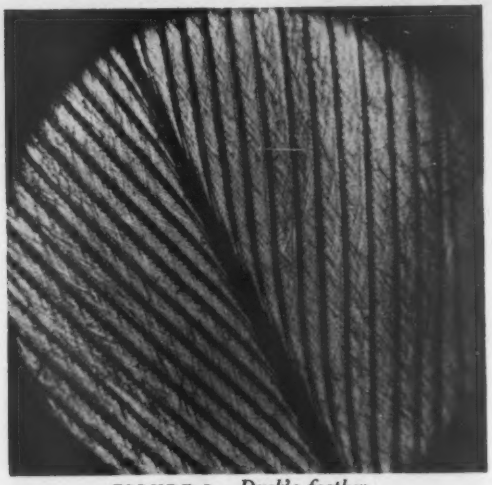


FIGURE 1 – *Duck's feather.*

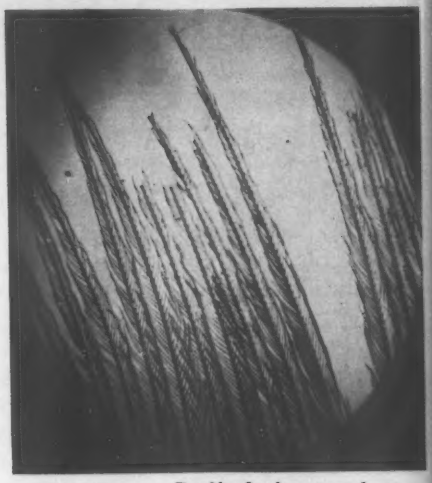


FIGURE 2 – *Duck's feather, teased out.*

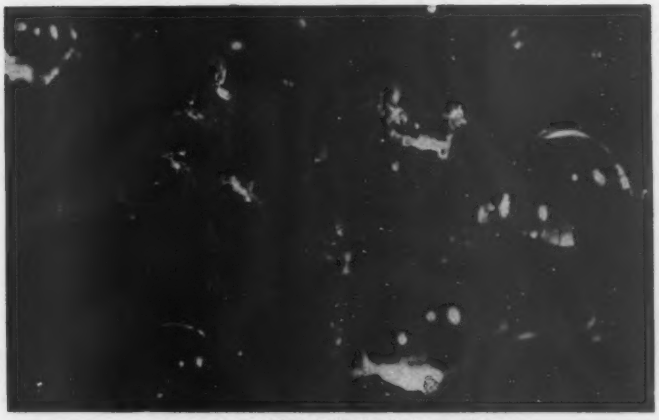
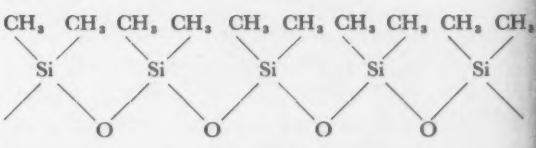


FIGURE 3 – Salvinia *leaf, from above.*

FIGURE 4 – *Projections on* Salvinia *leaf, in profile.*

Surfaces of solid long-chain fatty acids show an angle usually between 100° and 105°. But if a solid mass of stearic acid is cut with a knife [3], the angle may be anywhere between 50° and 105°. This is because crystalline stearic acid consists of plane leaflets two molecules thick, with the water-attracting carboxyl groups sandwiched together between the long hydrocarbon chains. If the knife has travelled parallel to these leaflets, only hydrocarbon groups are exposed, since the adhesion between two hydrocarbon groups is very much smaller than that between two carboxyl groups. If, however, the knife has travelled across the leaflets, some carboxyl groups are exposed at the surface, which thus attracts water more strongly and gives a lower contact angle than does the pure hydrocarbon surface.

The attraction for water of polar groups buried below the surface extends for only a very short distance. Normally hydrophilic or water-attracting surfaces can be made almost as water-repellent as paraffin wax if they are covered with a film only one molecule thick of a long-chain fatty acid [4], or amine [5], or a heavy-metal soap. Treatment of a surface with a silicone,



which deposits an exterior layer in which only a single layer of methyl groups intervenes between water and the —Si—O—Si—O—Si—O—Si—

structure, raises the contact angle to such an extent that water collects in small drops instead of forming a film over the surface.

Replacing the hydrogen atoms by fluorine appears, from the rather few published observations of contact angle, to decrease the attraction for water. Polytetrafluorethylene (P.T.F.E. or Teflon) gave an angle of 108°, 14° higher than for polyethylene [6], and a monolayer of perfluorodecanoic acid, $C_9F_{19}COOH$, on copper [7] gave an angle of 102°. It seems probable that on a perfluorinated paraffin wax, water would show an angle slightly greater than 110°.

The highest contact angle obtainable with any natural smooth surface is therefore not likely to be much over 105°. This would be sufficient to provide quite good water repellency if it could be relied upon not to decrease on prolonged contact with water or exposure to a high concentration of water vapour. Unfortunately water soaks to some extent into almost all solids, slightly even into paraffin wax, so that the contact angle on a continually wetted surface decreases more or less rapidly. This decrease in angle after contact with water is one, though probably not the only one, of the causes of the so-called 'hysteresis' of contact angles. The 'advancing' angle obtained when the liquid is advancing over a dry surface is larger, sometimes by a considerable amount, than the 'receding' angle obtained when the liquid recedes from a previously wetted surface. For permanent water repellency the receding angle must stay large, preferably not below 90°, otherwise there is a tendency for the water to remain behind as a more or less continuous film instead of 'pearling' off the surface as mobile drops. It is very noticeable with many rainproofed fabrics that at the beginning of a shower raindrops can easily be shaken off; but later the whole surface becomes wet, and eventually rain may start penetrating right through the pores of the cloth. On a smooth surface an initial contact angle even of 105–110° does not give a sufficient factor of safety to achieve permanent water repellency.

Some plants and animals overcome this difficulty by developing surfaces with broken-up structures. It was noticed in the 1920s that roughness increases the contact angle if the material is of such a nature that it would give an angle greater than 90° if the surface were smooth. If, however, the angle on the smooth surface is less than 90°, roughness of the surface decreases the angle. Simply roughening a surface exaggerates the amount by which the natural contact angle differs from 90°. This effect was given quantitative form by R. N. Wenzel [8]; if r is the roughness factor, i.e. the ratio of the real to the apparent surface, the average contact angle $\bar{\theta}$ on the rough surface is given by

$$\cos \bar{\theta} = r \cos \theta \qquad \ldots.(4)$$

where θ is the angle on a similar but smooth surface.

In 1944 A. B. D. Cassie and S. Baxter [9] worked out the theory of average or apparent contact angles on surfaces where there are regularly spaced air gaps, as in the feathers of birds. They explained for the first time how the proverbial water-shedding property of the duck's back is due to the combination of a wax layer having a contact angle not much over 90°, with a special structure which the duck maintains by careful preening.

If for each square centimetre of the apparent surface the liquid actually touches solid over a total area f_1 cm², and the air gaps in the surface total f_2 cm², the average contact angle $\bar{\theta}$ is given by

$$\cos \bar{\theta} = f_1 \cos \theta - f_2 \qquad \ldots.(5)$$

θ is here the 'real' angle on a smooth surface with the same attraction for water as the solid parts of the composite surface. Equation (5) holds for both advancing and receding angles.

The fractions f_1 and f_2 were calculated for a set of parallel cylinders of diameter $2r$, regularly spaced with centres $2(r + d)$ apart; these are shown in section in figure 5. Figure 6 shows the average advancing angles $\bar{\theta}_A$ plotted against $(r + d)/r$ for various values of θ_A. Similar, but not identical, curves were obtained for the average receding angles $\bar{\theta}_R$. In both cases the average

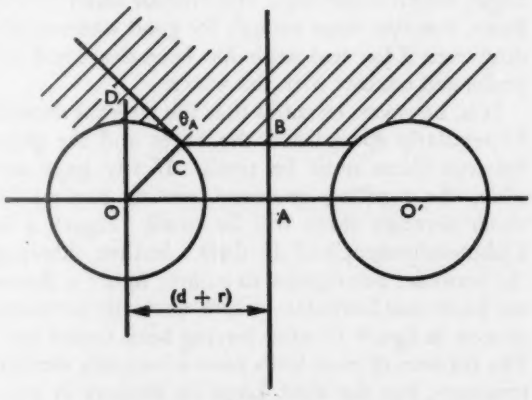


FIGURE 5 – *Calculation of f_1 and f_2.*

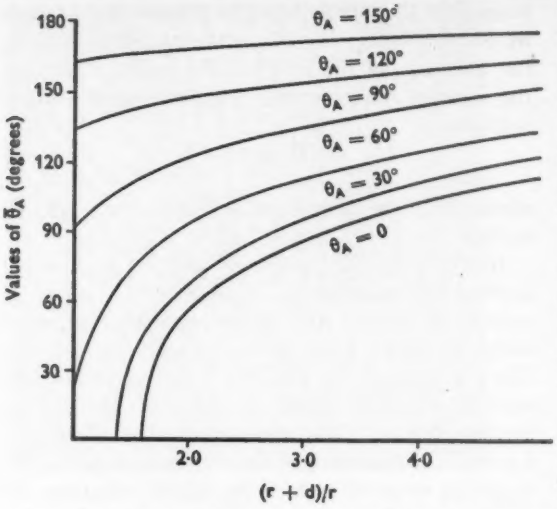


FIGURE 6 – *Values of average contact angle* $\bar{\theta}_A$ *for various values of* θ_A *and* $(r + d)/r$.

angle increases rapidly as the gaps between the fibres grow larger. For a set of parallel wires with diameter 0·07 mm, coated with a paraffin wax which on a smooth surface gave a 'real' advancing contact angle $\theta_A = 105°$ and a receding angle $\theta_R = 93°$, the average advancing angle $\bar{\theta}_A$ was 145° and the average receding angle $\bar{\theta}_R$ was 138°. The width of the gaps was in this instance about $2\frac{1}{2}$ times the diameter of the wires. The measured angles were very close to the calculated. A striking feature of the curves in figure 6 is that the average angles may be greater than 90° even when the real angles are far less. Thus the air gaps not only increase the initial contact angle, when the water first touches the surface, to an extent which makes it far easier for the drops to run off, but provide a 'factor of safety' whereby the average contact angle, which is the angle effective for water repellency, remains large enough for good water-shedding even if the real angle has been decreased by prolonged contact with the water.

It is, however, essential that the air gaps should be regularly spaced and the fibres and the gaps between them must be small. If any gaps are wide, the capillary pressure resisting passage of water through them will be small. Figure 1 is a photomicrograph of the duck's feather, showing the compact but regular structure; figure 2 shows the barbs and barbules—which normally interlace as seen in figure 1—after having been teased out. The feathers of most birds have a basically similar structure, but the duck keeps its feathers in particularly good condition. The domestic hen's feathers often have the gaps blocked with a continuous film, which impairs the water repellency. The barbules of the breast feather of the duck are about 8 μ in diameter, and the ratio $(r + d)/r$ is roughly 5. The barbules carry hooks and notches which interlock and prevent the barbules being drawn together by the surface tension of water adhering to the surface.

The contrast between fur and feathers is very marked: in fur the fibres are not held at their ends and therefore gather into compact tufts when wet. The spaces between the fibres are thus eliminated and the water repellency is reduced to that of a continuous surface.

The aquatic plant *Salvinia* sheds water perhaps even more effectively than the duck. Figures 3 and 4 show that the surface of the leaves is furnished with projections whose outer end is a loop of hydrophobic material. Water in contact with the leaf touches solid over only a small part of its area, so that in equation (5) f_1 is small and f_2 large. The result is that even after prolonged immersion the *Salvinia* leaves appear practically dry on removal from the water.

Some aquatic insects utilize a similar principle to enable them to remain under water almost indefinitely and to make use of dissolved oxygen for respiratory purposes. Much of their body surface is covered with very fine hydrophobic hairs which are nearly perpendicular to the surface for most of their length, but bend over at their tips so as to be nearly parallel to the surface. This layer of hairs entraps a practically constant volume of air, so that a thin layer of air is in permanent contact both with the insect surface and with the water. The hairs and the layer of air constitute the 'plastron', which functions as a kind of physical gill although there is no continuous membrane separating the water from the air. The structure

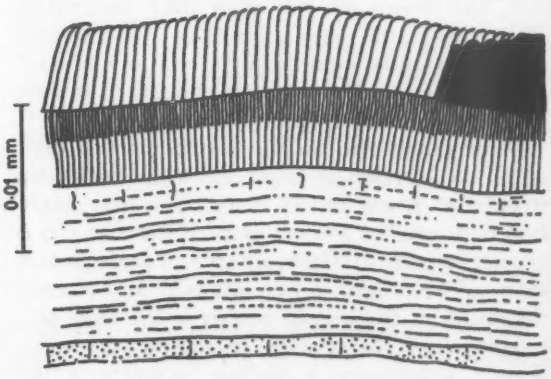


FIGURE 7 – *Plastron of* Aphelocheirus.

and functioning of the plastron have been very thoroughly studied by W. H. Thorpe and D. J. Crisp [10]. Figure 7 shows, diagrammatically, the structure of the plastron of the water-bug *Aphelocheirus aestivalis*. The hairs are about 0·18 μ diameter and the gaps between the hairs about 0·4 μ, corresponding to over two million hairs per square millimetre.

A pressure of at least two atmospheres is required to force water through the very fine mat formed by the tips of the hairs, and even if the surface tension and contact angle are substantially decreased by the presence of some surface-active impurity in the water, water has still great difficulty in penetrating into the air space. The rigidity of the hairs prevents appreciable decrease in the volume of the air in the plastron. Respiration occurs by absorption of oxygen from the plastron, this being replaced by diffusion of oxygen from the water outside. The plastron enables the insects to live almost permanently under water, and in this respect is much more efficient than the air bubbles which some insects attach to their bodies so as to provide a supply of air. Such air bubbles are not protected against changes in pressure, and they decrease in volume as the oxygen passes out of them into the insect and the nitrogen into the surrounding water. Insects that rely on attached bubbles for their supply of air under water have at intervals to come to the surface or to seek fresh bubbles under water.

The arrangement and shape of the minute hairs in *Aphelocheirus* is ideal for retaining a thin layer of air whose volume does not change. If the hairs were perpendicular to the surface over their whole length, they would entrap air only so long as the contact angle of the water with the hairs remained greater than 90°.

These considerations suggest possible ways of improving the waterproofing of textiles. Improvements are likely to depend on two factors. Firstly, the finding of substances which absorb water less than the best present agents, so that the contact angle will for a longer time remain large enough for drops to pearl off, instead of forming a continuous film. Secondly, devising a structure in which the position of threads and air gaps is maintained with such stability that the threads cannot draw together and obliterate some gaps, leaving others larger than when the fabric is dry.

ACKNOWLEDGMENT

Figures 5 and 6 are reproduced from the 'Transactions of the Faraday Society' and figure 7 from the 'Journal of Experimental Biology,' by kind permission of the Editors.

REFERENCES

[1] YOUNG, T. *Phil. Trans.*, **95**, 82, 1805.
[2] FOX, H. W. and ZISMAN, W. A. *J. Colloid Sci.*, **7**, 432, 1952.
[3] ADAM, N. K. and JESSOP, G. *J. chem. Soc.*, 1863, 1925.
[4] LANGMUIR, I. *Trans. Faraday Soc.*, **15**, 69, 1920; BLODGETT, K. B. *J. Amer. chem. Soc.*, **56**, 495, 1934.
[5] SHAFRIN, E. G. and ZISMAN, W. A. *J. Colloid Sci.*, **4**, 571, 1949; **7**, 166, 1952.
[6] FOX, H. W. and ZISMAN, W. A. *Ibid.*, **5**, 514, 1950.
[7] SCHULMAN, F. and ZISMAN, W. A. *Ibid.*, **7**, 470, 1952.
[8] WENZEL, R. N. *Industr. Engng Chem.*, **28**, 988, 1936.
[9] CASSIE, A. B. D. and BAXTER, S. *Trans. Faraday Soc.*, **40**, 546, 1944.
[10] THORPE, W. H. and CRISP, D. J. *J. exp. Biol.*, **24**, 227, 1947.

Radioactive tracers and fertilization in mammals

R. G. EDWARDS and J. L. SIRLIN

The importance of understanding the development of the spermatozoon and the ovum, and of the embryo resulting from their fusion, needs no emphasis. The experimental difficulties are, however, considerable. The use of radioactive tracers makes it possible to follow labelled spermatozoa within the egg and to study the interactions between nucleus and cytoplasm.

Knowledge of the processes of fertilization and early embryonic development in mammals has considerably increased in recent years. New techniques, such as the examination of living eggs and embryos under the phase-contrast microscope [1] and the incorporation of radioactive tracers into eggs and ovaries, have improved upon previous methods. Our intention here is to show how the use of tracers in studies on gametogenesis (i.e. the production of eggs and spermatozoa), fertilization, and early embryonic development is contributing to knowledge of the physiology of reproduction in mammals. These studies relate mainly to the mouse.

The principal technique used to detect and locate tracers incorporated into tissues has been autoradiography. From autoradiographs (figures 1–3), the localization of tracer inside a cell can be resolved to within about 2 μ. This permits the identification of labelled cells as compared with unlabelled cells in the same tissue. Certain tracers are incorporated into the developing sex cells in the seminiferous epithelium of the testis or in the ovary, and are retained in the cells during their maturation into gametes; it is therefore possible to follow the progress of spermatogenesis in the testis, or of oogenesis in the ovary. The labelled spermatozoa or eggs that result can then be used in studying fertilization and early embryonic development. The resolution of the autoradiographs is also sufficient to study in detail the intracellular distribution of tracer, and thus the technique has become an important tool in cell biochemistry.

SPERMATOGENESIS

Two days after an intraperitoneal injection of phosphorus-32 as phosphate into male mice, the tracer in the testis was incorporated mainly into spermatogonia (the first stage of development into spermatozoa) and remained in the cells during their development through the successive stages of spermatogenesis [2]. More recently other tracers, methionine-^{35}S [3] or adenine-8-^{14}C [4, 5], have been used in preference to phosphorus. Adenine also was incorporated mainly into spermatogonia soon after the injection [5]. It may also be taken up by spermatocytes (the next stage in spermatogenesis), for this tracer is incorporated into deoxyribonucleic acid (DNA), and the last synthesis of DNA during spermatogenesis occurs in young spermatocytes [6]. Methionine was incorporated into both spermatogonia and spermatocytes [3]. These tracers, especially adenine, were retained in the cells throughout spermatogenesis [2–5], and thus the progress of spermatogenesis could be timed by means of autoradiographs of the testes of males killed at various intervals after injection of the tracer.

The estimates obtained by this method were: 5 days from the most mature spermatogonia to the first meiotic prophase (spermatocytes) [5]; 8 days to early spermatids [5]; 18–25 days to spermatozoa free in the testis and epididymis [3–5]; and 30 days to ejaculated spermatozoa [4, 7]. The last figure was obtained by, at regular intervals, mating males injected with adenine and making autoradiographs of the ejaculated spermatozoa. The autoradiographs were negative, i.e. no tracer was detectable in the spermatozoa, until approximately 30 days after the injection of adenine. Labelled spermatozoa, i.e. spermatozoa showing a positive autoradiograph, as shown in figure 1, occurred in increasing numbers from 30 days after adenine was injected up to a maximum at 38 days, thereafter decreasing steadily [4, 7]. This increase is probably due to an increasing number of spermatogonia being labelled for some days after the injection [5, 7].

Spermatozoa can be labelled with formate-^{14}C, but this is much less effective than adenine [7].

FIGURE 1 – *Autoradiograph of mouse spermatozoa labelled with adenine-8-^{14}C. The photographic grains can be seen in the emulsion overlaying the sperm-heads; not all grains are in focus.* (× 900)

Glycine and methionine did not label individual spermatozoa to any significant extent [7]. The difference between adenine and the other tracers must arise from the fact that in mature spermatozoa adenine is incorporated mainly into DNA, which is relatively abundant and remains metabolically stable after meiosis. Some adenine may also have been incorporated into the mid-piece of the spermatozoon, though in very small amounts as compared with the sperm-head [7].

The tracer method is therefore of value in estimating the intervals between spermatogonia and the later stages of spermatogenesis. Because the tracers are incorporated into all three types of spermatogonia (A, intermediate, and B) they are not of direct use in timing the development of the different types. However, other means of estimating these intervals are available. The different types of cells in the seminiferous epithelium do not occur at random but in characteristic associations, and the relative duration of each association of cell types has been estimated by determining their frequencies [8]. The absolute durations have been estimated by X-irradiating males and studying the course of the subsequent progressive damage in the seminiferous epithelium. Spermatogonia B are relatively the most easily destroyed by X-rays, and this results in the progressive disappearance from the seminiferous epithelium of the subsequent stages of spermatogenesis [9, 10]. By killing males at various intervals after irradiation and then finding the cell associations from which particular spermatogenic stages have been eliminated, the time taken for each type of spermatogonia to mature, and also for later stages of spermatogenesis, was estimated. A disadvantage is that X-irradiation may retard spermatogenesis, though probably not excessively.

By the X-irradiation method, the interval between the development of spermatogonia A and the appearance of spermatozoa free in the testis, has been estimated as 34·5 days in the mouse [10], and the intervals between the stages of spermatogenesis have also been measured. There is reasonable agreement between the estimates obtained by this method and those obtained with tracers [7]. By combining the results of both methods, the total duration of the spermatogenic cycle—from spermatogonia A to ejaculation—has been estimated as 42 days [7].

The measurement of the relative durations of cell associations in the seminiferous epithelium of the ram [11] has been combined with tracer technique [12]. This combination reduces the possibility of a delay in spermatogenesis due to irradiation, particularly when tracers are used that emit soft radiation. The prophase of the first meiosis in the ram was calculated to take 15 days, spermiogenesis 14 days, and the total duration of spermatogenesis was 49–51 days [12]. After injecting phosphorus-32 into cocks, the radioactivity of seminal plasma was a maximum after one week [13], and of spermatozoa after two [13] or more [14] weeks.

THE USE OF LABELLED SPERMATOZOA

Labelled spermatozoa assist study of the fate of spermatozoa in the female reproductive tract, for by the use of autoradiography their presence can be detected even though they may have lost their typical shape and become unrecognizable under the microscope. In mouse eggs fertilized by labelled spermatozoa, only one of the two pronuclei was labelled [15]. The labelling must have been of the male pronucleus, and by this identification the results of previous investigations by other workers into the morphology of the male and female pronuclei were confirmed. Some tracer which was found in the cytoplasm of these eggs has provisionally been ascribed to the path of sperm entry into the egg [15]. Male chromosomes could be identified by their autoradiograph in the prophase of syngamy [7]. Labelled spermatozoa were also seen between the cumulus cells that surround the eggs [15].

Some naturally occurring or experimentally treated eggs do not possess two pronuclei after fertilization. In one egg which contained three pronuclei, two of them were labelled and the egg was therefore dispermic [15]. This illustrates the value of labelled spermatozoa in the identification of pronuclei, though the number of male and

female pronuclei can generally be more easily determined by counting the number of sperm-tails in a living egg under the phase-contrast microscope. In eggs possessing only one pronucleus, however, identification of the pronucleus is best achieved by using labelled spermatozoa, for it is difficult to gain this information from living eggs.

If some of the many excess spermatozoa which fail to fertilize the eggs of the female do nevertheless fulfil a useful function—e.g. if they penetrate into and become part of the uterus, the fallopian tubes, or other tissues ('somatic fertilization')—they should be detected in autoradiographs of these tissues. No positive autoradiographs of these tissues have so far been detected in mice, however [7], and this supports other evidence against the reality of somatic fertilization [16]. Many of the excess spermatozoa are removed from the uterus by leucocytes; in the uterine contents of females previously mated to injected males some labelled spermatozoa were found ingested by leucocytes [7]. Other leucocytes were labelled though no spermatozoa could be seen inside them, the ingested spermatozoa having apparently been destroyed.

Spermatozoa form only a small part of the ejaculate, the bulk of it being seminal plasma. By injecting other tracers into males (glycine-2-^{14}C, methionine-^{35}S), the seminal plasma was labelled without appreciable labelling of the spermatozoa [17]. Labelled seminal plasma was ejaculated for only a few days after injection of the tracers, and its activity had decreased considerably 10 days after injection [17]. Labelled seminal plasma could be used in experiments similar to those described above for labelled spermatozoa, and the distribution in the female tract of the two components of the ejaculate could then be compared. No convincing evidence has so far been found of tracer in the eggs of females mated to males ejaculating labelled seminal plasma: either the seminal plasma did not enter the eggs at all or it entered in amounts too small to be unequivocally identified by autoradiographs [17].

MATURATION OF THE EGG

Detailed studies using tracers have been made of the physiology of the ovary and the oocyte. The granulosa cells of rabbit, rat, and mouse will incorporate phosphorus-32 [17–20], glycine-2-^{14}C [17], adenine-8-^{14}C [17], sulphate-^{35}S [21–24], or methionine-^{35}S [24] in considerable amounts (figure 2), as a result of the high mitotic rate [20, 25] and secretory function of this tissue. Atretic follicles

FIGURE 2 – *Autoradiograph of a sectioned mouse ovary labelled with glycine-2-^{14}C. The granulosa (G) and, to a less extent, a young oocyte (A) are labelled. (Reproduced by permission of the publishers of the Proceedings of the Second World Congress on Fertility and Sterility, Naples, 1956.)* (× 900)

incorporate less tracer than normal follicles [18, 20, 22]. Autoradiographs reveal the position of tracer atoms of phosphorus and labelled glycine that are incorporated into proteins and nucleic acids; of adenine incorporated into nucleic acids; of methionine incorporated into protein; and of sulphate incorporated mainly into mucosaccharides. The incorporated ^{32}P has been found to be present in either DNA or ribonucleic acid (RNA) [17, 20]. Caution must be exercised in interpreting data derived solely from phosphorus-32, for

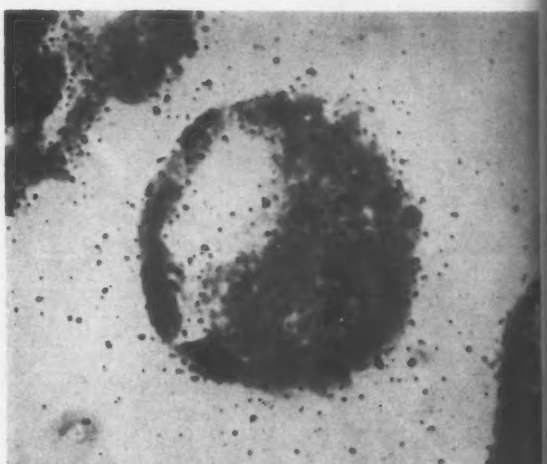


FIGURE 3 – *Autoradiograph of a sectioned mouse blastocyst labelled with glycine-2-^{14}C.* (× 1300)

the doses necessary to obtain autoradiographs are bound to damage tissue.

The incorporation of phosphorus, adenine, and glycine into the granulosa was very high 36 hours after the injection of tracer into females, but declined rapidly in a few days [17]. Phosphorus and glycine, but not adenine, labelled the follicular fluid to a certain extent; from this difference, and from the rate of loss of tracer from the fluid, it was concluded that the fluid is in equilibrium with the serum [17; see 26]. Sulphate-^{35}S is secreted into the follicular fluid in considerable amounts, probably as a sulpho-mucopolysaccharide [27], after its accumulation in the granulosa [22–24]. The amount of incorporated sulphur-35 depended on the physiological activity of the ovary: the amount in the follicular fluid was greatly increased if the ovarian follicles were stimulated by the advent of puberty or by injections of gonadotrophins, and this response may be of value in assaying follicle-stimulating substances [28]. At ovulation, ^{35}S left the rabbit follicle with the follicular fluid, being probably contained in mucins that assist in ovulation and the transport of eggs down the fallopian tubes [27]. Sulphate was not accumulated into luteal cells of the rabbit [29] or rat [22], though the growing connective tissue of mature and old corpora lutea in the rabbit incorporated moderate and high amounts respectively [29].

Incorporation of tracer was always less into the oocyte than into the granulosa (figure 2), but was still sufficient to allow study of the detailed distribution of tracer in the various cell constituents. From three hours after the injection of phosphorus, glycine, or adenine into mice, small concentrations of tracer were present in the cytoplasm; more was in the nucleus and nuclear wall; and large amounts were in the nucleolus and its wall [17, 19]. These cytochemical observations accord with current ideas on the sites of synthesis of proteins and nucleic acids in the cell. After longer intervals from the injection, more activity was found in the cytoplasm of the oocyte, and this was probably contributed by the granulosa [17]. In the rabbit, phosphorus was concentrated around the periphery of the egg, possibly in the zona pellucida [31], though this was more uncommon in the mouse oocyte [19]. The fallopian tubes of the rabbit secreted large amounts of sulphur-35 (injected as sulphate) associated with mucins, which reinforced the tracer already on the egg membranes [22, 24, 32]. Part of the sulphur-35 in tissue may have been in the form of sulphydryl complexes [32].

In contrast to the labelling of spermatozoa, labelling of mouse eggs was detectable for only a short period after injection of tracers [17]. This difference—probably due to the labelling in the eggs of a more labile chemical system that retained tracer for only a few days—made it impossible to follow the development of particular generations of eggs, as was possible with spermatozoa, and the technique has so far been of no value in timing the stages of oogenesis. Nevertheless, the considerable amount of tracer incorporated into the ovulated eggs before fertilization has been useful in studying fertilization and embryonic development.

DEVELOPMENT OF LABELLED EGGS

Labelled eggs have been used to study some aspects of fertilization and embryonic development. After injection of adenine-8-^{14}C [17], glycine-2-^{14}C [17], phosphorus-32 [17], or methionine-^{35}S [33] into mice twelve hours before ovulation, considerable activity was present in the ovulated eggs. When labelled eggs are fertilized by non-radioactive spermatozoa, all the tracer in the pronuclei must come from the egg. In mouse eggs, the male and female pronuclei each possessed approximately equal amounts of tracer [17], the cytoplasm of the egg having apparently contributed to the growth of the male pronucleus. In these pronucleate eggs, tracer was concentrated in the cytoplasm near the nuclear wall, indicating enhanced synthesis at that site together with, or alternatively, migration of nuclear products; this supports previous observations on autoradiographs of amphibian and chick embryos [30]. Thus a general pattern of the synthesis of protein and nucleic acids appears to hold for various animal classes. In the majority of the pronucleate eggs, approximately one-quarter of the egg had less activity than the remainder [17]; this quarter may correspond to the vacuolated area which, on cytochemical evidence [34], contains no RNA and lies to one side of the polar axis.

Mouse eggs labelled with glycine or methionine have been sectioned at intervals during development from the two-celled stage to blastocysts [17, 33]. In these stages, information was obtained about the intracellular distribution of tracer. The graded distribution of RNA detectable by cytochemical methods [34] in stages between two-celled eggs and morulae could not be identified from the distribution of tracer in eggs labelled with glycine [17], but this gradient might have been obscured by the incorporation of glycine into compounds other than RNA. Labelled eggs and embryos have been transplanted into the uteri of

females carrying their own embryos of the same age, and into females with no embryos of their own [17, 33], in attempts to identify the transplanted radioactive embryos and to distinguish them from the non-radioactive embryos of the host at later stages of gestation. The labelled embryos were distinguishable in the two-celled stage [33], had sufficient activity remaining for identification as blastocysts at 3½ days gestation [17] (figure 3), but were not identifiable at 7½ days gestation because the tracer had become too diffuse to produce an autoradiograph [17]. The labelling of embryos therefore permits the maternal origins of two types of embryos in the same uterus to be identified up to 3½ days gestation; this is otherwise extremely difficult.

An alternative method of labelling mouse eggs has been attempted [17]. Small amounts of glycine-2-^{14}C were injected through the cervix into the uterus of superovulated mouse females at the time of ovulation, and the females were then mated. The fertilized eggs recovered were found to be labelled; tracer could have entered the eggs either with the spermatozoa or by absorption during passage through the fallopian tubes. Some evidence for the latter was found in that blastocysts were more heavily labelled than pronucleate or two-celled eggs; this would be expected if continued residence in the tubes resulted in the uptake of more tracer. The activity in the embryos was always less than that found after intraperitoneal injections of tracer into females, though smaller amounts of tracer were injected through the cervix [17]. These results indicate the possibility of labelling embryos *in vitro* using small amounts of tracer.

Each developed grain in the autoradiographs represents essentially the radiation from the decay of one radioactive atom. This radiation, originating in the cell itself or in its neighbourhood, could cause genetic changes in each cell of the embryo. Further, after decaying an atom has changed into an atom of a different chemical element, and this new element, produced before or after incorporation into chromosomes, could cause mutations or chromosomal aberrations. After the injection of tracer into females, embryos labelled with sulphate [35], methionine [33], glycine [36], or adenine [36] have developed to birth and maturity, all of them being apparently normal. Offspring bred from these animals to test for induced recessive mutations were also normal, though the experiment was on too small a scale to prove the absence of induced mutations [36]. These tracers emit soft radiations and did not harm the mothers [35, 36], but on the other hand radiation damage of a physiological rather than a genetic nature has been widely recorded from tracers emitting hard radiation, e.g. radio-iodine [37, 38] and radio-phosphorus [36]. Phosphorus-32 in relatively small doses resulted in radiation sickness in the mother and the total absence of offspring, while higher doses suppressed embryonic development to 3½ days gestation [36].

CONCLUSION

We have described how recent work with tracers and autoradiographs is enlarging the scope of studies on the maturation of gametes and the early development of mammals, especially the mouse. Tracer technique applied in studies on the laboratory mammals has been extended to the domestic mammals [11–14], though the price of tracers makes extensive research on these animals somewhat costly. Less expensive adaptations of the technique would therefore be desirable. As an example of a cheaper technique, mouse eggs and embryos have been labelled *in situ* by flushing the uterus and fallopian tubes with solutions containing less tracer than that needed to label ovarian oocytes by intraperitoneal injections [17]. Perhaps it may eventually prove possible effectively to label mammalian spermatozoa [39] and eggs either *in vitro* or by injecting tracers into testes and ovaries.

ACKNOWLEDGMENTS

We are indebted to Professor C. H. Waddington, F.R.S., for his interest in the work, and to Dr R. A. Beatty and Miss R. E. Fowler for helpful criticisms.

REFERENCES

[1] AUSTIN, C. R. and SMILES, J. *J. R. micr. Soc.*, **68**, 13, 1948.
[2] HOWARD, A. and PELC, S. R. *Brit. J. Radiol., N. S.*, **23**, 634, 1950.
[3] GLUCKSMANN, A., HOWARD, A., and PELC, S. R. *J. Anat.*, **89**, 13, 1955.
[4] SIRLIN, J. L. and EDWARDS, R. G. *Exper. Cell Res.*, **9**, 596, 1955.
[5] PELC, S. R. and HOWARD, A. *Ibid.*, **11**, 128, 1956.
[6] SWIFT, H. H. *Physiol. Zoöl.*, **23**, 169, 1950.
[7] SIRLIN, J. L. and EDWARDS, R. G. *Nature, Lond.*, **179**, 725, 1957, and in preparation.
[8] LEBLOND, C. P. and CLERMONT, Y. *Amer. J. Anat.*, **90**, 167, 1952.
[9] OAKBERG, E. F. *Radiation Res.*, **2**, 369, 1955. *J. Morph.*, **97**, 39, 1955.

[10] OAKBERG, E. F. *Amer. J. Anat.*, **99**, 391 and 507, 1956.
[11] ORTAVANT, R. *C. R. Soc. Biol., Paris*, **198**, 1958, 1954.
[12] *Idem. Arch. Anat. micr. Morph. Exper.*, **45**, 1, 1956.
[13] LORENZ, F. W., CAVOULAS, M., and CARSON, J. D. *Poult. Sci.*, **29**, 769, 1950. (*abs.*)
[14] TAKEDA, A. *Sci. Bull., Fac. Agric., Kyushu Univ.*, **15**, 391, 1955.
[15] EDWARDS, R. G. and SIRLIN, J. L. *Nature, Lond.*, **177**, 429, 1956.
[16] VOJTÍŠKOVÁ, M. *Folia Biol., Prague*, **2**, 293, 1956. POSALAKY, Z. and TÖRŐ, I. *Nature, Lond.*, **179**, 150, 1957.
[17] EDWARDS, R. G. and SIRLIN, J. L. *Proc. 2nd Wld Congr. Fertility and Sterility. Naples.* 1956.
[18] ODEBLAD, E. *Exper. Cell Res.*, **2**, 574, 1951. *Acta radiol., Suppl.*, p. 93, 1952.
[19] ODEBLAD, E. and MAGNUSSON, G. *Acta endocr., Copenhagen*, **17**, 290, 1954.
[20] LEBLOND, C. P., STEVENS, C. E., and BOGOROCH, R. *Science*, **108**, 531, 1948.
[21] ODEBLAD, E. *Exper. Cell Res.*, **3**, 694, 1952.
[22] BOSTRÖM, H. and ODEBLAD, E. *Acta endocr., Copenhagen*, **10**, 84, 1952.
[23] ODEBLAD, E. and BOSTRÖM, H. *Acta radiol.*, **39**, 137, 1953.
[24] MORICARD, R. and GOTHIÉ, S. *C. R. Soc. Biol., Paris*, **199**, 1918, 1955. *3rd Internat. Congr. Animal Reproduction, Cambridge*, Sect. I, 31, 1956.
[25] BULLOUGH, W. S. *J. Endocr.*, **3**, 150, 1942.
[26] BRAMBELL, F. W. R., in Marshall's 'Physiology of Reproduction', Vol. 1, pp. 447–48. A. S. Parkes (Editor). Longmans, Green & Co., London. 1956.
[27] ODEBLAD, E. *Acta endocr., Copenhagen*, **15**, 313, 1954.
[28] NATI, G. and ODEBLAD, E. *Ibid.*, **19**, 43, 1955.
[29] ODEBLAD, E. *Ibid.*, **11**, 306, 1952.
[30] SIRLIN, J. L. and WADDINGTON, C. H. *Exper. Cell Res.*, **11**, 197, 1956.
[31] AMINOFF, U., MAGNUSSON, G., ODEBLAD, E., and WETTERDAL, K.-H. *Ibid.*, **3**, 609, 1952.
[32] GOTHIÉ, S. and MORICARD, R. *C. R. Soc. Biol., Paris*, **199**, 2084, 1953.
[33] LIN, T. P. *Nature, Lond.*, **178**, 1175, 1956.
[34] DALCQ, A. M. *Rev. gén. Sci. pur. appl.*, **61**, 19, 1954.
[35] ODEBLAD, E. and BOSTRÖM, H. *Acta radiol.*, **41**, 525, 1954.
[36] EDWARDS, R. G. and SIRLIN, J. L. (Unpublished.)
[37] RUGH, R. *J. exper. Zool.*, **122**, 151, 1953.
[38] HUMPHREYS, E. M. *J. Endocr.*, **12**, Suppl., p. viii, 1955.
[39] BISHOP, D. W. 'Studies on Testis and Ovary, Eggs and Sperm'. E. T. Engle (Editor). C. C. Thomas, Springfield, Illinois. 1952.

Book reviews

EIGENVALUES

Variational Methods for Eigenvalue Problems, *by S. H. Gould. Pp. xiv + 179. University of Toronto Press, Toronto; Oxford University Press, London.* 1957. 48*s. net.*

This work is devoted to an exposition of Rayleigh-Ritz and Weinstein's methods for approximate calculation of eigenvalues. The eigenvalues of elliptic differential equations of mathematical physics may be considered either as possible values for a parameter in the equation itself, or alternatively and equivalently from the variational point of view as maxima and minima of certain expressions. The strength of the variational approach, which forms the subject of Professor Gould's book, lies in the facility with which a given problem can be modified into an approximately similar and soluble problem.

Once a given problem is cast in the variational form, one can do two things: one can introduce more stringent prescribed conditions and find minimum values of the parameter for the new problem. This gives upper limits for the eigenvalues of the original problem and is in essence the Rayleigh-Ritz method. On the other hand, one can weaken the prescribed conditions so that one obtains lower limits for the desired eigenvalues. This is the method of Weinstein. A combination of the two methods will confine the eigenvalue within close limits.

Professor Gould gives a clear and systematic exposition of the concepts involved, developing practically all the mathematics he needs. Notwithstanding the high standard of rigour, he achieves a readability equalled only by a few of the preceding volumes in this series. The book has been a pleasure to read and is warmly recommended to both elementary and advanced students.

A. SALAM

HISTORY OF ASTRONOMY

Discovery of the Universe, *by G. de Vaucouleurs. Pp.* 328. *Faber and Faber Ltd., London.* 1957. 30*s. net.*

It might not be inferred from the title that this book is a short history of astronomy from the earliest times to current developments. To compress such a history into about 300 pages demands careful selection of material, and in this the author has achieved a large measure of success. The book has been written with the emphasis on the development of ideas rather than on biographical details.

The first half of the book covers the period up to about the middle of the nineteenth century, during which period the history of astronomy can be adequately dealt with in chronological order, the number of those who contributed significantly to the development of ideas being then comparatively small. From the middle of the nineteenth century onward developments have become increasingly rapid, and it is more convenient to deal in turn with various branches of the subject. The second half of the book is divided almost equally between the period from the middle of the nineteenth century to the second world war, and from 1945 to the present date.

The author has endeavoured to cover a very wide field in this last period, so that the text becomes almost a catalogue of different investigations and loses something in interest. A useful selected analytic bibliography and both name and subject indexes are given. It is gratifying that the dates of birth and death of astronomers not now living are given, with few exceptions. H. SPENCER JONES

GALACTIC NEBULAE

Galactic Nebulae and Interstellar Matter, *by J. Dufay, translated by A. J. Pomerans. Pp.* 352. *Hutchinson's Scientific and Technical Publications, London.* 1957. 60*s. net.*

M. Dufay's treatise, *Nébuleuses galactiques et matière interstellaire*, has since its publication provided the most complete and authoritative account of diffuse matter in the Universe, in its various forms—atoms, molecules, and solid particles. It was well worth publication in an English edition. As the subject is in a state of rapid development, M. Dufay has, for the purpose of this edition, revised and extended certain sections, thereby providing a complete account of the information available about interstellar matter up to 1955.

This English edition is well printed and produced, and the plates are excellent. It is unfortunately, however, marred by many defects. The proofreading has been slipshod; there are errors in formulae and in proper names, as well as many misprints—such as monograms for nomograms, convention for convection, occulation for occultation. The punctuation is bad throughout. Many inappropriate adjectives are used, such as weak stars for faint stars, lightest stars for least massive stars, wide open objective for wide-aperture objective, small temperature for low temperature, large emission lines for broad emission lines, proper brightness for intrinsic brightness. Various terms are incorrectly translated: variable clusters instead of cluster variables; Schmidt chamber instead of Schmidt camera. Many sentences are just nonsense, such as 'the curve . . . has the same appearance as a point', 'thousands of invisible blue stars have been counted on the infrared negative'. The meaning in these cases can usually be inferred from the context, but not infrequently a passage is so unintelligible that it can be interpreted only by reference to the original French text. The construction of many sentences is extremely awkward so that the precision of the French text is completely lost.

All who are interested in this subject and who can read French are recommended to use the original French edition in preference to this poor translation. H. SPENCER JONES

GALACTIC NOVAE

The Galactic Novae, *by Cecilia Payne-Gaposchkin. Pp.* $x + 336$. *North-Holland Publishing Co., Amsterdam.* 1957. 30.50 *guilders net.*

The novae or temporary stars, which flare up suddenly, increasing in brightness many thousands or even millions of times, and then gradually fade again, are objects of special interest. The phenomena that accompany their outburst are of great complexity. Their spectra show rapid changes; in the early stages several spectra of different types and with different Doppler displacements are superposed. The normal nova increases in brightness by about 10 magnitudes, but exceptional novae —termed supernovae—show a much greater increase. The supernovae are divided into two classes, with increases of about 20 and 15 magnitudes respectively; their spectra differ in character from those of normal novae. No clue is available at present for the interpretation of the spectra of Class I supernovae, while hydrogen is the only element definitely identified in the spectra of Class II supernovae. In addition to the true novae there are many stars that show nova-like characteristics, which may be recurrent, but with a smaller rise in brightness.

There has hitherto been no book in English (although there is one in Russian) devoted entirely to this important class of stars. This excellent monograph fills a notable gap in astronomical literature. The number of papers that have been published dealing with individual novae or with various nova problems is extremely large. The author has missed no paper of importance, and each chapter ends with an extensive bibliography. The available information about all observed galactic novae is summarized, separate chapters being devoted to stars for which first-class data are available, to stars for which the data are second-class, and to stars for which the data are more fragmentary. Two chapters are devoted to the various classes of nova-like stars, and one to the supernovae. The concluding chapters are concerned with a comparative study of spectral development, and with evolutionary and theoretical problems. The nova outburst probably belongs to the final stages of a star's career; there have been many speculations about the underlying cause, but nothing is known with certainty. The author states that 'very likely no Type I supernova has appeared in our galaxy since 1604, and we may hope for another in the not distant future.' We may have to await such an occurrence for the interpretation of the complicated spectra of these stars of exceptional interest.

This book will be invaluable for reference purposes. Only a few errors and misprints, and those of a minor nature, have been noticed. The production and printing are excellent.

H. SPENCER JONES

AMERICAN UNIVERSITY PHYSICS

Currents, Fields and Particles, *by F. Bitter. Pp.* 599 + *xiv. The Technology Press of Massachusetts Institute of Technology and John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1956. 68*s. net.*

It is always of interest to study the teaching methods of another country as indicated by a well-written textbook. Undergraduate courses can readily become very stereotyped, and in Britain a nearly standard pattern prevails, at least during the first two years. It therefore comes as quite a surprise to find that this book devotes scant attention to geometrical optics. As one reads steadily forward, reaching electromagnetic waves by Chapter 8, the impression is gained that the level reached approaches that of the third year of an Honours course. Chapter 9, however, deals with Optics—reflection and refraction, which represent the uppermost level attained in this subject. It is merely that the relative weight and order assigned to different branches are markedly different from those in Britain.

In many respects Professor Bitter's treatment is more in keeping with modern requirements than is the normal undergraduate course in a British university. There has been in Britain too great a tendency to add new matter to the syllabus without correspondingly reducing the burden by adequate rationalization of the original material.

The special features claimed for this book include an early introduction to the concepts of mass particles, dualism, and the elements of atomic theory. It can be fairly said that in these respects

the treatment is good and could well be followed in the second year of an undergraduate course in a British university—a level that is roughly that of the book as a whole, due allowance being made for certain basic differences.

The book has been thoughtfully planned, is clearly the fruit of long teaching experience, is well illustrated and easy to read; there are numerous well-chosen examples at the end of each chapter. It would be a very useful and stimulating work for British students to possess, but the price is very high.

H. S. W. MASSEY

MODERN NUCLEAR PHYSICS

Elementary Nuclear Theory (*second edition*), *by H. A. Bethe and P. Morrison. Pp. xi + 274. John Wiley & Sons, New York; Chapman and Hall Ltd., London.* 1956. 50*s. net.*

One of the difficulties of the present hectic state of nuclear and high-energy physics is the scarcity of good books on the subject. Potential authors cannot spare time to write a book which may be largely out of date by the time it is published. The first edition of Professor Bethe's book was very welcome, but this new expanded edition in which Professor Morrison has collaborated is of even greater value.

In a remarkably compact fashion the authors have succeeded in covering most aspects of nuclear physics as it stood towards the end of 1955. The clarity with which often quite subtle arguments are expounded will certainly make the book essential for the research student beginning work on nuclear physics, the university teacher, or even specialists in either the experimental or theoretical side of the subject. Any physicist specializing in other fields who wishes to obtain an idea of the present state of development of nuclear physics would also be well advised to read this book.

The new edition, apart from having been generally expanded, includes new material on nuclear reactions and scattering and on the physics of mesons. Particular attention is paid to the discussion of polarization effects in nuclear collisions. This is both appropriate and useful, as in the last few years much attention has been paid to the experimental and theoretical study of these effects, with very substantial rewards as judged from applications to the interpretation of nuclear forces.

The work now includes three major sections: the description theory of nuclei (7 chapters), the quantitative theory of nuclear forces (11 chapters), and complex nuclei and β-decay (3 chapters). A table of nuclear species is given in the appendix, and there is also a table of numerical relations. H. S. W. MASSEY

NUCLEAR MOMENTS

Theories of Nuclear Moments, *by R. J. Blin-Stoyl. Pp. 88. The Clarendon Press. Oxford.* 1957. 8*s.* 6*d. net.*

This book is the first of a new series entitled 'Oxford Library of the Physical Sciences', which promises to be of great value to those engaged in scientific research. This volume sets a high standard.

The magnetic dipole moments of most atomic nuclei have now been measured to an accuracy of 1 per cent or better, and some nuclear quadrupole and octupole moments have also been determined. Dr Blin-Stoyl presents a critical discussion of the theoretical interpretations of these measurements.

It is at present quite impossible to obtain exact wave functions to represent the structure of a nucleus except for the very simplest cases, and so it is necessary to construct simplified models for which calculations can be made. The principles of the various models are outlined very clearly, followed by a detailed discussion of their use in interpreting measured values of nuclear moments.

This monograph represents a valuable stocktaking of the present position in this field for the expert, but the discussions are at the same time clear enough to be most useful to the research worker not directly concerned with these theoretical problems. R. E. RICHARDS

NEUTRON TRANSPORT

Neutron Transport Theory, *by B. Davison. Pp. xx + 450. Oxford University Press, London.* 1957. 75*s. net.*

At first sight the subject of this monograph does not seem to possess the generality of most of the subjects that have so far been treated in the series. The book, however, reveals surprising ramifications and the need for a considerable mathematical apparatus in dealing with the problems of neutron transport. The author, an acknowledged expert in his field, has drawn on both published and unpublished material to make this surely a standard text for many years to come.

The treatment throughout is not difficult, but the profusion of symbols and the fact that ideas are drawn from so many different fields do not make for easy reading. The more general reader would like to dip into the book at intervals and extract those parts that happen to be of value to him; but it must be admitted that the work does not lend itself to this, and in fact the understanding of its parts requires an understanding of the whole. For that reason it is doubtful whether it will have the general appeal that a similar, but less detailed, book might have had. For those whose primary interest is neutron transport theory it would be difficult to imagine a better book.

L. R. B. ELTON

SEMICONDUCTORS

Progress in Semiconductors, *Vol. II, edited by A. F. Gibson. Pp. vii + 280. Heywood & Co. Ltd., London.* 1957. 63*s. net.*

This is the second volume of a welcome series of reviews. Although there is some continuity between this and the previous text, the editors have wisely chosen to deal with other timely subjects in the field. Semiconductor alloys (Ge–Si) and the III–V intermetallic compounds are well covered, and their growing importance in the fundamental study of energy states in crystals is emphasized. Germanium is given the space of two articles, one on the production of single crystals of high purity and the other on the physical properties of germanium containing controlled concentrations of selected impurities. There are valuable treatments of radiation damage in semiconductors and of the problem of carrier lifetimes in such solids. An account of the effects of high-intensity electric fields on semiconductors is complemented by an article on the theories of electroluminescence.

Each review appears to be up to date and complete with adequate references to literature. Only a brief general index of main topics is included at the end of the text.

The editor is to be commended for his choice of both material and authors.

G. F. J. GARLICK

ELECTRICAL CONTACTS

The Physics of Electrical Contacts, *by E. Llewellyn Jones. Pp. x + 219. The Clarendon Press, Oxford.* 1957. 35*s. net.*

For many years the study of the phenomena associated with the working of electrical contacts has been regarded as chiefly the concern of the engineer: only comparatively recently have the fundamental physical aspects of the

problem been investigated in detail. Professor Llewellyn Jones's book is therefore particularly timely; he has avoided lengthy technological discussions and has presented a concise and lucid treatment of the basic physics of electrical contacts.

After giving a general review of the subject and of the results of earlier workers, Professor Llewellyn Jones selects for special attention the problem of fine transfer in medium-voltage contacts. He discusses the possible mechanisms, and in particular gives a detailed account of the asymmetry resulting from the Thomson effect. The discussion of the properties of metallic bridges at high temperatures provides an interesting illustration of how the fundamental study of a particular phenomenon can in turn lead to the use of that phenomenon as the basis of a new experimental technique.

The book ends with a disappointingly short chapter on the behaviour of electric contacts when closed (as distinct from that during make and break). Several aspects are mentioned briefly, but the chapter as a whole does not reach the high standard of the earlier parts of this book. J. B. P. WILLIAMSON

COVALENT BONDS

Chemistry of the Covalent Bond, *by L. B. Clapp. Pp. xiv* + 684. *W. H. Freeman & Co., San Francisco; Bailey Bros. and Swinfen Ltd., London.* 1957. $7.50 *net.*

This book is intended for the first-year course at an American university where the freshmen will have previously had one year of chemistry, little mathematics and physics, and no calculus. It deals with covalent substances, almost all being organic. Although best described as an elementary textbook of organic chemistry, it also contains a general introduction and chapters on oxidation and reduction, acids and bases, and the development of the concept of the covalent linkage. Unlike most elementary books, descriptions of laboratory methods are absent. Most chemists will probably consider this wrong, since chemistry, particularly in the fields dealt with, is usually regarded as essentially a practical subject. After reading this book, the student will have no idea of the preparation or handling of the substances described. The approach is entirely one of considering the properties in terms of the bonds. There are sections on strain theory, stereochemistry, resonance and electronic displacements. Probably in order to stimulate the interest of a certain type of student, there are references to technical applications and to proteins, colour, etc. The book provides an interesting, if unusual, introduction to organic chemistry for students.

J. W. LINNETT

LABORATORY SAFETY

Safety in the Chemical Laboratory (*second edition*), *by H. A. J. Pieters and J. W. Creyghton. Pp. xiv* + 305. *Butterworths Scientific Publications, London; Academic Press Inc., New York.* 1957. 40*s. net.*

As a work of reference on laboratory safety this book has much that is commendable, but the preface states that the book is 'primarily intended to be the property of every member of the laboratory staff. It should be used on the laboratory bench, not in the library'. The high price of the present edition makes this very improbable.

Far too much detailed attention is given to certain sections, e.g. analysis, while more important items are scarcely mentioned. Seven pages are devoted to protection of the individual, whereas five lines and one figure are allowed for screens. The most important pieces of safety equipment, however, are adequate screens. This must always be the first line of defence, protection of the individual being considered only because the guarding of the apparatus may not be entirely safe. Too much has been covered in one book, and the emphasis is placed on secondary items. The result is that the book is not large enough for a comprehensive manual on safety and too large for a bench safety handbook. L. J. BURRAGE

NON-ELECTROLYTE SOLUTIONS

The Molecular Theory of Solutions, *by I. Prigogine. Pp. xx* + 448. *North-Holland Publishing Co., Amsterdam.* 1957. 48 *guilders net.*

This book deals with the difficult problem of relating the equilibrium properties of liquid mixtures to the individual properties of the molecules and the forces between them. Considerable advances have been made in this field during the last two decades, partly stimulated by interest in polymer solutions. Recent books have concentrated on a semi-empirical approach to the problem ('Solubility of Non-electrolytes', by Hildebrand and Scott, 1950) or on the lattice model of the liquid state ('Mixtures', by Guggenheim, 1952); the present book places the main emphasis on the cell model, originally used by Lennard-Jones and by Eyring, and in particular on the average potential cell model which has been intensively studied by Professor Prigogine and his school during the last few years. This is not a book for the general reader, but it will be extremely valuable to those with a primary interest in non-electrolyte solutions, since the theory is often more clearly and logically developed than in the author's original papers on the subject. Comparison with experiment is stressed throughout, and the presentation and production are excellent. R. P. BELL

HETEROCYCLIC COMPOUNDS

Heterocyclic Compounds, *Vol. VI, edited by R. C. Elderfield. Pp. vii* + 753. *John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1957. £10 *net.*

This substantial volume is the latest addition to this well-known and much-valued series of reference books. The treatment of six-membered heterocyclic compounds containing two hetero-atoms follows the pattern of the previous volumes, and the importance of these ring systems is reflected in the size, and unfortunately also in the price of the volume.

The fourteen chapters deal with the following ring systems: dioxanes (Kremer and Rochen), benzodioxanes (Elderfield), sulphur analogues of dioxanes (Elderfield), pyridazines (Jacobs) cinnolines (Jacobs), phthalazine (Elderfield and Whyte), pyrimidine (Kenner and W. H. Todd), quinazoline (Williamson), pyrazines and piperazines (Pratt), quinoxalines (Pratt), oxazines (Cromwell), benzoxazines (Elderfield, Todd, and Gerber), thiazines (Elderfield and Harris), and phenazines, phenoxazines, and phenothiazines (Pearson). The major chapters are those on the pyrimidines, the pyrazines, the oxazines, and the phenazines.

As in the previous volumes, the treatment of the different groups is almost wholly concerned with their preparation and chemical properties. There is little discussion of physical properties, reaction mechanisms, or stereochemistry. The several thousand references are mainly from the fairly recent literature and greatly enhance the value of the book, as do the thousands of graphic formulae with which it is illustrated.

W. BAKER

GEOLOGY

Géologie sédimentaire: les séries marines, *by Augustin Lombard.* Pp. 722. *Masson et Cie, Paris.* 1956. *Frs.* 11 000.

Professor Lombard's large book deals with sedimentary rocks in relation to geology as a whole. This is a big undertaking, but the rhythms found in so many sedimentary rock sequences provide a central theme. No other work deals with this facet of geology in such detail, and the book is to be warmly welcomed.

The first of four parts deals with the present-day marine and lake sediments in relation to their environments. The second part begins the consideration of the marine sedimentary rock series. The third part, on sedimentary associations, forms the core of the book, especially a section of eighty pages dealing with the rhythms shown by sedimentary rocks. Use is made of the concept of ideal rhythmic sequences of various kinds, with which the actual, incomplete, and often accidentally confused sequences are compared. Finally, the genesis of the sedimentary rock series in relation to tectonics and depositional environment is considered.

Professor Lombard's book is not an easy one to evaluate. He has presented his material from an unconventional point of view, which in itself is good. The author emphasizes his own views and hypotheses by printing them in italics, while other conceptions of greater general significance are not highlighted. The book will undoubtedly prove stimulating, but its price is high, and many geologists might have preferred a less bulky book at lower cost.

L. R. WAGER

EVOLUTION AND RELIGION

Evolutionary Theory and Christian Belief, *by David Lack. Pp.* 128. *Methuen & Co. Ltd., London.* 1957. 10*s.* 6*d.*

Dr Lack has already written some very attractive studies of bird life. But here he turns to the broader problems of evolution and its possible conflict with Christian belief. As befits one who is Reader in Ornithology at Oxford and Director of the Edward Grey Institute, it is a most carefully documented study. The author writes as one for whom the essential validity of the theory of evolution is now beyond dispute, and yet as one for whom the acceptance of this theory, so far from solving problems, opens new ones. As the sub-title of the book ('The Unresolved Conflict') suggests, Dr Lack is unwilling to fall down on either side of the fence, though he very cogently points out how impossible it must always be to accept the significance of thoughts and judgments made by a human mind if that mind is nothing but the product of some evolutionary process. For once in a way here is an absolutely fair book: from the nature of things it is not a very profound work, and can be read by anyone with just a sprinkling of scientific knowledge. The whole story of the so-called conflict between Darwinism and Christianity has been bedevilled by so much bad spirit, and bad history, that it is refreshing to see both sides of the dispute stated so clearly and reasonably.

C. A. COULSON

PURINES

The Chemistry and Biology of Purines (Ciba Foundation Symposium), *edited by G. E. W. Wolstenholme and Cecilia M. O'Connor. Pp. xii* + 327. *J. and A. Churchill, London.* 1957. 48*s. net.*

For biochemistry this is the era of the study of metabolic and biosynthetic pathways, an inevitable outcome of the application of radioisotopes to biochemical research. The purines have received their full share of attention—one might even say a liberal share—since one of the most promising approaches to the cancer problem appears to be through a detailed understanding of purine synthesis, nucleic acid formation, and the precise relationship of nucleoproteins to protein synthesis. The pteridines are closely involved in some of these transformations, and it is understandable that they also are the subject of much activity.

The volume under review sets on record a Ciba Symposium attended by the leading research workers in the fields of purine and pteridine chemistry and biochemistry. The emphasis is largely chemical, and a debt of gratitude will be owed by many to the sponsors of the symposium and to the publishers, whose volume provides an authoritative and up to date exposition of the work going on and of results achieved in this vitally important field. The high standard of presentation of previous volumes is maintained, the clarity of formulae and figures deserving special praise.

C. RIMINGTON

THYROID HORMONES

Ciba Foundation Colloquia on Endocrinology, *Vol. X*, Regulation and Mode of Action of Thyroid Hormones, *edited by G. E. W. Wolstenholme and Elaine C. P. Millar. Pp. xii* + 311. *J. and A. Churchill Ltd., London.* 1957. 48*s. net.*

The study of thyroid hormone metabolism is being pursued so actively and in so many laboratories that it is unusually difficult to obtain a survey of the subject which is both comprehensive and up to date. A Ciba colloquium was held in June 1956 at which the control of thyroid hormone release and the biochemical nature of the hormones' action and metabolism were discussed with the fullness, freshness, and authority characteristic of these meetings. The papers and their discussion as now published are outstandingly valuable for the review that they give of this rapidly developing subject. As one would expect, this is a collection of ideas, suggestions, and comments as well as a very full survey of results; the discussions following each paper preserve their verbatim form, although sufficiently edited by their authors to appear articulate in print. The biochemistry of these hormones, their possible active forms, and their renal and hepatic metabolism are particularly well and authoritatively dealt with in papers discussed or presented by Pitt-Rivers, Roche and Michel, Thibault, Gross, Taurog, Maclagan and Wilkinson, Lardy, Vannotti, and Lissitzky: names which in themselves indicate the value and quality of this aspect of the colloquium. The papers of Halmi, Barker, Querido, and Courrier all also contain essentially new points of biochemical importance in thyroid metabolism; the pituitary control of the gland's activity is the subject of further papers given by Purves, Greer, and G. W. Harris. For those working on the biochemistry of the thyroid this book will be essential, and it is likely to form a useful and suggestive survey for those working in many fields of metabolism and of endocrine function.

E. E. POCHIN

NUCLEIC ACIDS

Biochemical Society Symposia No. 14: Structure of Nucleic Acids and their Role in Protein Synthesis, *edited by E. M. Crook. Pp.* 74. *Cambridge University Press, London.* 1957. 20*s. net.*

This is the collection of papers presented for a Biochemical Society symposium in London in February 1956. The collection reviews and discusses the evidence for the structures of nucleic acids and the role of these acids in protein synthesis. It begins with a brief

discussion of the recent chemical and physical evidence for the proposed structures of ribonucleic and deoxyribonucleic acids. Cytological aspects and the extensive metabolic evidence for the involvement of nucleic acids in protein synthesis in animal tissues and bacteria are then presented. The collection ends with a discussion of the role of deoxyribonucleic acid in bacteriophage multiplication.

This book summarizes much of this important phase of biochemistry up to 1956 and emphasizes some of the very recent work done in the United Kingdom on these problems. The collection provides a concise, well-presented, and useful review of the interrelation of nucleic acids and protein synthesis and will certainly be of interest to those workers who are turning to these problems. D. GROSS

ENZYMOLOGY

Advances in Enzymology. Vol. XVIII, edited by F. F. Nord. Pp. v + 435. Interscience Publishers Inc., New York; Interscience Publishers Ltd., London. 1957. $9 *net.*

Biochemists who look forward to the annual appearance of volumes of authoritative and topical reviews of various aspects of enzymology and related subjects will not be disappointed with this most recent addition to the series.

This volume contains two excellent complementary reviews on 'Cytochrome in Higher Plants' by E. F. Hartree and on 'Reaction Paths in the Respiration of the Higher Plants' by W. O. James. Though there is some unavoidable overlap between these two chapters, readers wishing to acquire up-to-date detailed knowledge of interesting progress in plant biochemistry will be glad to have the two points of view in one volume.

A. Kornberg's article, 'Pyrophosphorylases and Phosphorylases in Biosynthetic Reactions', gives a wealth of information about reactions of many enzymes with phosphate esters, including those involved in *in vitro* synthesis of nucleic acids.

R. A. Peters, in 'Mechanism of Toxicity of the Active Constituents of *Dichapetalum cymosum* and Related Compounds', describes an interesting chapter in biochemistry—the effect of fluoroacetate on cell metabolism—to which he himself has contributed so much. 'Newer Knowledge of Succinic Dehydrogenase' by T. P. Singes and colleagues is timely and valuable, as is 'The Chemistry and Function of Lipoic Acid' by L. J. Reed. An interesting article, *Le rôle biosynthétique du cycle des acides tricarboxyliques* by J. M. Wiame, and reviews on 'Deoxyribonucleoproteins' by J. A. V. Butler and P. F. Davison and on 'Lignification' by W. J. Schubert and F. F. Nord complete this valuable and well-produced volume. H. GUTFREUND

GROWTH SUBSTANCES

Symposia of the Society of Experimental Biology, No. 11: The Biological Action of Growth Substances, *edited by H. K. Porter. Pp. vi + 344. Cambridge University Press, London.* 1957. 55*s. net.*

This symposium is concerned with the physiology of growth substances in plants and animals; their chemistry and biochemistry received only incidental attention. On the botanical side, in particular, it can be regarded as somewhat of a landmark, since it closely followed a period of exciting new discoveries. The spotlight naturally falls on the plant cell-division factors or kinetins, and on the gibberellins with their astounding modifications of plant form. It was also a time for the exposure of old myths, and one must thank Dr J. R. Raper for his masterly exorcism of Moewus's elaborate system of algal sexual hormones that has bedevilled botanists for so long. The current theories of apical dominance and phototropism also come in for some healthy criticism by Gregory and by Brauner respectively. The volume contains a number of less exciting yet nevertheless very valuable summaries of current views of the part played by hormones in plants. The papers on the animal growth substances do not evoke the same appeal of new discovery, although they are extremely valuable statements of the present picture in its many aspects. Particularly readable is the account by Wigglesworth of growth hormones in insects and the discussion by Klein and Klein of the evolution of independence of mammalian tumour cell populations.

The production of the volume is of the technical excellence we have long come to associate with the symposia of the Society of Experimental Biology. This new volume contains such a wealth of ideas and information that it will be indispensable to all workers in this and associated fields. L. J. AUDUS

VISUAL PIGMENTS

The Visual Pigments, *by H. J. A. Dartnall. Pp. vii + 216. Methuen & Co. Ltd., London; John Wiley & Sons Inc., New York.* 1957. 30*s. net.*

The visual pigments occur in the retina of the eye, and their light sensitivity is basic to vision. This book is a critical survey of the experimental methods used, the results obtained, and the theories put forward in the study of the nature of the pigments and their reaction with light. A large part of the book is concerned with visual purple (rhodopsin), which is a compound of protein with a derivative of vitamin A and is the best-known light-sensitive pigment of the vertebrate retina. After a welcome introductory chapter describing experimental methods there are chapters on the visual pigments and their photoproducts, on the physical and structural chemistry of visual purple, and on the part played by different stereoisomers of vitamin A in the photochemistry of the different pigments. These are followed by chapters in which the methods of assessing homogeneity of visual pigment solutions are discussed, and the book ends with a description of experimental work carried out on visual pigments in the retina rather than after extraction into solution. This is the first comprehensive book on the visual pigments for a very long time, and the author is to be congratulated on his balanced account of the subject. He has not over-simplified the evidence and leaves us with the feeling that though a great deal is now known, there are still many uncertainties, and generalizations may be premature. A. PIRIE

PLANT PATHOLOGY

Plant Diseases due to Bacteria (*second edition*), *by W. J. Dowson. Pp. xv + 232. Cambridge University Press, London.* 1957. 32*s.* 6*d.*

Until the first edition of this work appeared in 1949 there was a noticeable gap in the field of plant pathology covered by British textbooks and manuals. The publication of this second edition after as short an interval as seven years justifies the confidence felt by competent judges as to the high standard of the work and the wide need for the book by research and advisory plant pathologists as well as by students of biology and bacteriology. The subject matter has been generally brought up to date, and ample

reference is made to investigations into the etiology and control of bacterial diseases of crop plants carried out since 1949. Care has also been taken to include the latest microchemical tests used in determinative bacteriology, and mention is made of the use of specific phages for bacterial identification. While the format of the work remains almost unchanged, a short new chapter on the preservation of bacterial cultures has been added. The photographs, although reduced in number from forty to thirty, are ample and well chosen, while the re-inclusion of the series of crop disease distribution maps published by the Commonwealth Mycological Institute is a welcome feature. The book is very well produced, and there are few errors. A. E. MUSKETT

MAMMALIAN DEVELOPMENT

Parthenogenesis and Polyploidy in Mammalian Development, *by R. A. Beatty. Pp. xi + 132. Cambridge University Press, London.* 1957. 15*s. net.*

There is no doubt that two fields of inquiry to which biologists nowadays are giving increasing attention are the detailed cytology of early development and the study of spontaneous or induced irregularities in chromosome number. That these apparently unrelated subjects are in fact truly complementary and gain much in comprehensibility when considered together is amply demonstrated in Dr Beatty's book. The author achieves a notable synthesis, setting out clearly the genetic consequences of the modifications that can occur during the initiation of a new individual. Variation arises in different ways: through heteroploidy of spermatogonia or oogonia, through failure of abstriction of either or both polar bodies, through abnormalities of fertilization and cleavage, or through non-specific activation of the egg. Since development may proceed under one or more of these circumstances, numerous possible combinations exist, and the multiplicity of results would become hopelessly confusing were it not for the skilful and systematic manner in which Dr Beatty handles his material. A large volume of published data, from non-mammalian as well as mammalian species, is critically examined and classified, and the text thus provides a unique source of evaluated information. Several significant implications are discussed, and the incisive conclusions given add further to the value of this important little treatise. C. R. AUSTIN

ENTOMOLOGY

A General Textbook of Entomology *(ninth edition), by A. D. Imms, revised by O. W. Richards and R. G. Davies. Pp.* x + 886. *Methuen & Co. Ltd., London.* 1957. 75*s. net.*

This book was first published in 1925 and the last complete revision was made in 1934. To bring the work up to date while retaining its general form unchanged was a formidable task which has been magnificently accomplished by Professor O. W. Richards and Dr R. G. Davies. The whole book has been expanded by about one-quarter. The number of orders recognized has increased from 23 to 29. Classification at the family level has been extensively revised in almost all groups, and new keys for the identification of families have been prepared. Many new references have been added, and these are now set out in a standard and extended form at the end of each chapter. The physiological sections did not really represent current knowledge, even in 1925; they have now been completely rewritten and provide a condensed and accurate account of present ideas on the physiology of insects. Since its first appearance 'Imms' has held its position as the most useful general account of entomology. In its new form it is still further improved.

V. B. WIGGLESWORTH

APES AND MAN

Primates, Part 3—Pithecoidea, Platyrrhini, Hapalidae, *by W. C. Osman Hill. Pp. xix* + 354. *Edinburgh University Press, Edinburgh.* 1957. 90*s. net.*

With this third volume Dr Hill has arrived taxonomically roughly at the half-way stage of his monumental survey of the Primates. He begins here his review of the higher suborder of the monkeys, apes, and man, which collectively he calls the *Pithecoidea*, though what is to be gained by a resurrection of this term in place of the currently accepted *Anthropoidea* is by no means clear.

In accordance with his general plan for the whole series Dr Hill provides descriptions at each hierarchical level, working down from the suborder *Pithecoidea* (= *Anthropoidea*) and the infra-order *Platyrrhini* to the families and the various genera of the Marmosets and Tamarins. This arrangement imposes some repetition, but in practice it is often of great value to have these comprehensive definitions without recourse to an inordinate amount of cross-reference. Indeed, Dr Hill has done his work conspicuously well in the way these taxonomic descriptions have been dovetailed.

The present volume will be especially welcomed, as there has long been a noticeable lack of good systematic generic descriptions of the New World monkeys. One cannot readily agree, however, with the taxonomic and nomenclatural changes which Dr Hill continues to introduce in his survey. His discussions are not always sufficiently critical to persuade the reader to accept many of his rearrangements. Despite these reservations, there is no doubt that Dr Hill's work has already an assured place as an indispensable guide to the morphology and distribution of the primates. J. S. WEINER

MEDICINE AFLOAT

Medicine and the Navy 1200–1900, *by J. J. Keevil. Vol. I,* 1200–1649. *Pp. xii* + 255. *E. and S. Livingstone Ltd., Edinburgh.* 1957. 40*s. net.*

Under the terms of their trust, the Wellcome Trustees are empowered to give assistance 'for the collection of information of every kind connected with the history of medicine and allied sciences.' In sponsoring the present work they are using their powers to admirable purpose, for—strange as it may seem, especially to a seafaring nation—no systematic study has previously been made of the part played by medicine in the lives of those who go down to the sea in ships. Surgeon Commander Keevil is well equipped to provide such a study, and in this volume, the first of four, he treats of the early period from 1200 to 1649. Starting with the introduction of the laws of Oléron into England at the opening of this period, he goes on to describe—among much else—the provision made for sick and injured seamen, the *ad hoc* engagement of surgeons for service 'with the king's lieges sailing . . . to Calais' in 1436, the ravages of disease in both the Spanish and the English fleets at the time of the Armada, and the large share of responsibility borne by sickness for the naval decline that followed the first Elizabethan age.

The royal ships were in fact sadly neglected from the medical point of view, and the author stresses the fact that the origins of naval medicine are mainly to be found in the voyages of privateers and merchantmen. Such perspicacious adventurers as Drake

gradually acquired considerable knowledge of the conditions necessary for healthy life at sea, together with experience of tropical diseases, and in due course the importance of having competent medical help on board won tardy recognition. Even by 1649, however, the state of affairs was very far from satisfactory, and we must await Mr Keevil's subsequent volumes to learn how it was improved. This first instalment of the work is absorbingly written, fully documented, and well illustrated. Most appropriately, it is dedicated to Charles Singer. E. J. HOLMYARD

PLANT BREEDING

Plant Breeding and Genetics in India, *Vol. I, by R. H. Richharia. Pp. vii + 410. Scientific Book Co., Patna, India.* 1957. 30*s. net.*

This book, with slight modifications, is a reprint of a work with the same title published in 1945. In it a survey is made of the considerable literature on plant breeding and genetics, embracing the principal economic crops of India. It deals only with the researches of workers in India, and in this respect is exceptionally complete from the bibliographical point of view. There is, however, no reference to the studies of workers in other countries, although such studies have often covered the same ground and arrived at the same conclusions. It is thus difficult for either Indian or foreign readers to tell where foreign work left off and Indian work began, and vice versa, and considerable historical confusion results.

The information provided, for example, on the genetics of *Ricinus* with reference to the work of Patwarden on capsule spininess and waxiness was anticipated many years previously by work in the West Indies and the United States. Surely Indian workers should know this. It may be true that the haploid number of chromosomes of the garden pea was determined by Roy in 1933 as 7, but this fact was common knowledge many years previously.

The author has nevertheless assembled a large amount of useful information, and has done service both to his Indian colleagues and to foreign workers. It is his intention to publish a further volume dealing with work done in India after 1945. S. C. HARLAND

WILLIAM HARVEY

William Harvey. His life and times; his discoveries; his methods, *by Louis Chauvois. Pp.* 271. *Hutchinson Medical Publications, London.* 1957. 25*s. net.*

Around the rather thin materials for the life of Harvey, Dr Chauvois, a well-known French historian of medicine, has woven a fresh narrative of the career and discoveries of the great physiologist. Excellently translated into English and amply illustrated, it is a useful and interesting volume; even the author's excursions into purely imaginary incident are made tolerable by his obvious enthusiasm. Earlier researches by D'Arcy Power, H. P. Bayon, Geoffrey Keynes, and others, which have brought to light almost everything that is known of Harvey as a man and fully investigated his stature as a scientist, have been skilfully used. Dr Chauvois' most abrupt dissent from the views of these authorities is in the importance he attaches to Cesalpino as a precursor of Harvey. In Cesalpino's writings he finds 'set forth quite clearly a circulation from arteries to veins and then from veins to arteries', and though Dr Chauvois admits that Harvey nowhere refers to Cesalpino, his 'reconstruction' supposes that Harvey could have derived his idea from the Italian naturalist. The extensive quotations adduced to support this opinion do not seem as clear as the author's interpretation of them. Despite this, however, Dr Chauvois sets Harvey's achievement at the highest level, and justly explains its fundamental significance for medical science. His biography can be commended to all whose interest has been aroused by the recent celebrations, though for the scholar it will hardly replace the older books. A. R. HALL

HISTORY OF NAVIGATION

The Haven-Finding Art. A History of Navigation from Odysseus to Captain Cook, *by E. G. R. Taylor. Pp. xii* + 295. *Hollis and Carter, London.* 1956. 30*s. net.*

The myth that early sailors navigated by hugging the shore dies hard, even though, as Professor Taylor points out, 'every sailor has a wholesome dread of being driven on to a lee-shore and stands well out to sea to avoid the dangers . . . of inshore waters.' She amasses ample evidence that long before the Christian era, when the sounding-line was the sole instrumental aid to navigation, sailors sought the relative safety of the open sea and regularly ventured far out of sight of land. In recounting that Odysseus sailed by the stars, Homer was evidently referring to a practice even then quite familiar to sailors.

From these early beginnings she tells the story of how men navigated at sea until the time of Cook, perhaps the greatest navigator of all time. She describes not only instrumental aids—ranging from the *kamal* and traverse-board to the mariner's compass and Harrison's chronometer—but such other related topics as charts, pilot books, and tide-tables. All this is combined with a very clear account—written with remarkable insight into the seaman's problems—of how the instruments were, and in many instances still are, used at sea. This is an exceptional book: at once both a work of serious scholarship and a very readable account of a subject of great interest and importance.

TREVOR I. WILLIAMS

RENAISSANCE SCIENTISTS

Six Wings: Men of Science in the Renaissance, *by George Sarton. Pp. xiv* + 318. *Indiana University Press, Bloomington.* 1957. $6.75 *net.*

This is a study based upon the late George Sarton's Patten Lectures delivered at Indiana University in 1955. The 'six wings'—a scholarly allusion to the six parts of the astronomical tables of the medieval writer Immanual Bonfils, and thence to the six wings of the seraphim in the Book of Isaiah—are the divisions of the book, which in characteristic fashion ranges over all parts of Renaissance science, medicine, and technology. The emphasis is upon the personalities involved rather than upon the investigations they made: in pursuance of this aim the book is illustrated with a considerable number of contemporary portraits. To say that the whole is erudite but eminently readable, far-ranging but not desultory, profound but never pretentious, is superfluous when we refer to Sarton: it is sad indeed that so powerful an intellect is with us no more. Among so much that is good it is invidious to praise any feature in particular, but many will find special enjoyment in the final 'wing', which is a brilliant comparative study of Leonardo da Vinci and Albrecht Dürer. Clearly, Sarton felt spiritual affinity with Leonardo: 'Dürer was always practical and earth-bound; Leonardo was the opposite . . . I admire them both but I love Leonardo.'

TREVOR I. WILLIAMS

Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

A Guide to the Literature of Chemistry (*second edition*), *by E. J. Crane, Austen M. Patterson, and Eleanor B. Marr. Pp. xiv* + 397. *John Wiley & Sons Inc., New York; Chapman and Hall Ltd., London.* 1957. 74*s. net.*

A proper understanding of how and where to seek information in the vast literature is of the greatest importance to all chemists. Since 1927 Crane and Patterson's 'A Guide to the Literature of Chemistry' has been a standard work. It now appears in a revised and modernized form, the revision being largely the work of Dr Eleanor B. Marr. It is an effective guide to books, journals, patents, government publications, trade literature, libraries, and other sources of chemical information.

Dictionary of Scientific Terms (*sixth edition, revised and enlarged by John H. Kenneth*), *by I. F. Henderson and W. D. Henderson. Pp. xvi* + 532. *Oliver and Boyd, Edinburgh.* 1957. 32*s. net.*

The scope of this work is considerably less than its title suggests, for it is in fact a dictionary of terms used in the biological sciences. Within these limits it ranges widely, and some fourteen thousand terms are defined. This is a well-established reference book, for the first edition, prepared by I. F. and W. D. Henderson, was published in 1920 and was itself somewhat on the lines of Stormonth's 'Manual of Scientific Terms', which first appeared in 1879. The present (sixth) edition, prepared by J. H. Kenneth, represents a substantial revision and expansion of the last (1953) edition.

The Science Museum: the First Hundred Years. *Pp.* 85. *Her Majesty's Stationery Office, London.* 1957. 15*s. net.*

The Science Museum at South Kensington, one of the fruits of the Great Exhibition of 1851, was first opened to the public in 1857. This book has been published to mark the centenary. A short introduction recounts the history of the museum, and the remainder of the book describes and illustrates twenty-one of its principal exhibits. The latter range from the famous Wells Cathedral clock of 1392 to Parsons' steam turbine of 1891; from Boulton and Watt's rotative beam engine to Joule's apparatus for measuring the mechanical equivalent of heat.

The United Kingdom Contribution to the International Geophysical Year 1957–58. *Pp.* 72. *The Royal Society, London.* 1957. 10*s. net.*

The International Geophysical Year involves the co-operation of some sixty nations. The arrangements for the United Kingdom contribution have been entrusted to the Royal Society, which in this book describes what is to be done. The book describes in some detail the organization of the work, the contributions of the various disciplines involved, and the geographical distribution of the United Kingdom stations. The final chapter describes the establishment and programme of the Royal Society's base at Halley Bay in Antarctica.

New Intermediate Physics, *by G. R. Noakes. Pp. viii* + 961. *Macmillan & Co. Ltd., London; St Martin's Press, New York.* 1957. 30*s. net.*

This is a comprehensive survey of physics designed for students preparing to enter British universities. The sections on general physics and on heat are substantially the same as the author's earlier books on these subjects; the sections on light and on electricity were inspired by earlier books but have been entirely rewritten. In the section on light a wave treatment is used throughout. The M.K.S. system of units is used.

Sztuka Spoleczenstw Paleolitycznych, *by Witold Hensel. Pp.* 158. *Panstwowe Muzeum Archeologiczne, Warsaw.* 1957. 42 *zlotys net.*

This is a short account—handsomely illustrated in colour, half-tone, and line—of the art of palaeolithic communities. It treats the subject from many points of view: the distribution and nature of examples of palaeolithic art, the materials and implements used, the evolution of artistic thought, and the functions of this primitive form of art. The main text is in Polish, but there are short summaries in Russian and English.

Practical Microscopical Metallography (*fourth edition*), *by Richard H. Greaves and H. Wrighton. Pp. xiv* + 221. *Chapman and Hall Ltd., London.* 1957. 70*s. net.*

This is the fourth edition of a standard work on the methods and results of the examination of metals by means of the optical microscope. While this edition follows the same lines as its predecessors, the opportunity has been taken for revision and enlargement.

Men, Missiles and Machines, *by Lancelot Hogben. Pp.* 69. *Rathbone Books, London.* 1957. 17*s.* 6*d. net.*

Power is an overwhelmingly important factor in modern civilization: it has been stated with a good deal of truth that a nation's standard of life is proportional to the amount of power available per head of its population. This beautifully produced and illustrated book—the fifth in a now well-known series—describes for the layman the basic principles governing the production and utilization of power and the various devices that are used for the purpose.

Rectifying Semi-conductor Contacts, *by H. K. Henisch. Pp. xii* + 372. *The Clarendon Press, Oxford; Oxford University Press, London.* 1957. 70*s. net.*

This is a specialized account of the phenomena observed at contacts between metals and semi-conductors, and it includes a detailed description of experimental methods. While a considerable knowledge of physics and mathematics is assumed, the introductory chapters are designed to assist those who have no detailed knowledge of the concepts and terminology of solid-state physics.

Notes on contributors

W. H. McCREA,
M.A., Ph.D., Hon. D.Sc., F.R.S.,

Was born in Dublin in 1904 and educated at Chesterfield Grammar School and the Universities of Cambridge and Göttingen. After holding posts in the University of Edinburgh and Imperial College of Science, London, he was appointed Professor of Mathematics at Queen's University, Belfast, in 1936. Since 1944 he has been Professor of Mathematics in the University of London at Royal Holloway College. He was a Bye-Fellow of Gonville and Caius College, Cambridge, during 1952–53 and Visiting Professor of Astronomy at the University of California in 1956. His work has been mainly in theoretical astrophysics and in relativity theory and its applications to cosmology.

KENNETH M. SMITH,
C.B.E., D.Sc., Ph.D., F.R.S.,

Was born in Helensburgh, Scotland, and educated at Dulwich College and London University. He was Senior Lecturer in Agricultural Entomology in the University of Manchester from 1920 to 1927. He was then appointed senior research assistant at the Potato Virus Research Station, Cambridge, where he remained until 1939. From that date he became director of the station, which is now the Virus Research Unit of the Agricultural Research Council. He has published several books on plant viruses, including a 'Textbook of Plant Virus Diseases' and 'Recent Advances in the Study of Plant Viruses'; he is also the author of 'Beyond the Microscope'.

ROBLEY C. WILLIAMS,
A.B., Ph.D.,

Was born in California in 1908 and was educated at Cornell University. His professional training was as a physicist, but his first university position was that of astronomer at the University of Michigan. In 1945 he transferred to the Physics Department, and was appointed Professor of Physics in 1949. He moved to the University of California in 1950, where he holds an appointment as Professor of Biophysics in the Department of Biochemistry and as biophysicist to the Virus Laboratory. During 1956–57 he was on sabbatical leave at the Virus Research Unit, Agricultural Research Council, at Cambridge. His research interests in recent years have been primarily in the electron microscopy of viruses. In 1955 he was elected a member of the U.S. National Academy of Sciences.

E. N. DA C. ANDRADE,
D.Sc., Ph.D., LL.D., F.R.S.,

Was born in London in 1887 and was educated at St Dunstan's College, the Universities of London, Manchester, and Heidelberg, and the Cavendish Laboratory, Cambridge. He served as an artillery officer in France in the war of 1914–18, and afterwards was for some years professor at the Artillery College (later the Royal Military College of Science). In 1928 he was appointed Quain professor of physics in the University of London and established a flourishing school of physics, known for fundamental work on the mechanical properties of the solid and liquid state. In 1950 he was appointed Director in the Royal Institution of Great Britain and Director of the Davy Faraday Research Laboratory, but in 1952 resigned, like his predecessor. He maintains close connection with colleagues overseas, and is *Membre Correspondant de l'Académie des Sciences, Institut de France.*

R. P. BELL,
M.A., B.Sc., F.R.S.,

Was born in 1907 and educated at Maidenhead County Boys' School and Balliol College, Oxford, to which he returned as a Fellow and Tutor in 1932 after four years' research in Copenhagen. He is University Reader in Physical Chemistry. He has worked mostly on reaction kinetics in solution, especially acid-base catalysis, and is also interested in general theories of solution and the application of quantum theory to chemical problems. He was Vice-President of the Chemical Society from 1948 to 1951 and President of the Faraday Society from 1956 to 1957.

N. K. ADAM,
M.A., Sc.D., F.R.S.,

Was professor of chemistry at University College, Southampton (now the University of Southampton), from 1937 to 1957. He was educated at Winchester and Trinity College, Cambridge, where he was a Fellow from 1915 to 1921. From 1921 to 1929 he was Sorby Research Fellow at the University of Sheffield, and from 1930 to 1936 was on the staff of Imperial Chemical Industries Limited and Honorary Research Associate at University College, London. He was lecturer in chemistry at University College, London, from 1936 to 1937.

R. G. EDWARDS,
B.Sc., Ph.D.,

Was born in Leeds in 1925 and graduated at the University College of North Wales, Bangor. In 1951 he entered the Institute of Animal Genetics, University of Edinburgh. Having received a research appointment from the Agricultural Research Council, since 1955 he has continued to work at the same Institute on the physiology of reproduction of mammals.

J. L. SIRLIN,
Dr. Nat. Sci.,

Was born in Buenos Aires in 1926, and studied at the Universities of Buenos Aires, Montevideo, and Santiago de Chile. In 1953 he came as a British Council scholar to the Institute of Animal Genetics, University of Edinburgh, where he is now employed by the British Empire Cancer Campaign. His work has dealt with the cytochemistry of embryos and the genetics of the mouse.